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ART. I. *On the best Method of warming and ventilating Houses and other Buildings.* *By* MR. CHARLES SYLVESTER.

[Communicated by the Author.]

[We are happy to comply with the request of several of our Correspondents, and to enter upon the subject of heating and ventilating our apartments; it is a subject upon which few persons venture to think for themselves, and is too frequently conceded to the management of the ignorant, or, what is worse, is intrusted to some half-informed speculator. The combustion of smoke is another branch of this inquiry, and, although but lightly touched upon in the following paper, shall not be forgotten. The absurdities which have of late been authoritatively thrust upon the public in relation to it, are so gross, as to merit more serious and extended notice than it is in our power to bestow upon them; but we shall humbly contribute whatever is within our reach to rectify the errors that have been diffused, and to show the inanity of the promises, with which this subject has lately been ushered into notice.]

THE action of the sun's rays on the surface of the earth, and the consequent accumulation of sensible heat is a most instructive lesson, for the best mode of applying artificial heat for warming buildings; and our best ideas of ventilation are derived from those mechanical changes in the atmosphere occasioned by the rarefaction of the air, from the heat it acquires in contact with the earth's surface. If the earth were perfectly transparent, or had a surface capable of perfect reflection, it would not be at all heated by the sun's rays; and our atmosphere, supposing it to exist under such circumstances, would be destitute of those changes which are daily evinced in an infinite variety of currents. If the substance of the earth were a much better conductor of heat, we should experience less extremes of heat and cold upon its surface. The summer-heat would be more rapidly absorbed by the earth, and the rigour of winter would be much diminished by the heat derived from the earth in the sun's absence. The nature of soils, as regards their conducting power, has doubtless a great influence in limiting the extremes of temperature in winter and summer. The heat produced on any part of the earth's surface, will be the greatest where the rays of the sun are vertical, and the surface of such a nature as to receive the rays with the greatest facility, its substratum being, at the same time, the worst conductor of heat. The air immediately in contact with this surface becomes heated, and specifically lighter than its superstratum. This causes, in the first instance, two simultaneous currents; one perpendicularly upwards, and the other, a lateral one from all the surrounding parts towards the centre of the heated surface. After the ascending current has attained a certain altitude, it progressively assumes an oblique and ultimately a lateral direction, but in an inverse order to that of the lower stratum. By this beautiful provision of natural economy, the heated air of the torrid zone, and the chilling currents from the polar regions mutually contribute to the prevention of those extremes of heat and cold, which would otherwise be fatal to every class of animated beings.

To form some idea of the effect which would result from a vertical sun upon a good reflecting surface, such as a black soil, unattended by the currents of air above alluded to, we have only to observe the heat generated in hot-houses; in which case the heated air is to a certain degree prevented from ascending, and consequently the lateral current from coming in. The heat produced by these means, therefore, will be greater in proportion to the blackness and lightness of the soil, to the tightness of the surrounding walls and windows, and the perpendicularity of the sun's rays. Hence we see the importance of our atmosphere independently of its agency in respiration. Without it, bodies would receive their heat on those parts only which are exposed to the direct rays, and would become unequally heated in the inverse ratio of their conducting power.

When bodies are immersed in a heated medium, such as in air or water, they receive their heat on every side; and it has been found by experience, that this mode of applying heat is of particular importance in the economy of animals and vegetables.

Nothing can be more unphilosophical than the common mode of warming ordinary rooms by open grates. To put an extreme case of this mode of warming, we have only to instance the effect of making a fire in the open air. In this instance, there is free access for the ascent of the rarefied current, and the lateral current rushing towards the fire is felt on every side, supposing no natural breeze prevailed. The effect of this cold current is so conspicuous on the human body, that few unaccustomed to such exposure would escape some variety of those affections called colds.

Our common dwellings approach this extreme case in proportion to the size of the fire, the width of the chimney, and the access of cold air by the doors and windows. In every case, as much cold air must be admitted as will effect the combustion of the fuel, and supply the demands of respiration. The air which would be barely sufficient for these purposes, coming immediately from a cold atmosphere into rooms with grates even of the best construction, will ever be a barrier to that comfort which we ought to experience, and which by the aid of other means can be easily attained.

Notwithstanding the absolute necessity of admitting a certain portion of fresh air into every room, it is a common practice with builders to make doors and windows so tight as frequently to be the sole cause of a smoky chimney. To obviate this evil, some have let in a certain quantity of atmospheric air under or near to the fire grate. By this expedient, those sitting around the fire are not annoyed by the cold current, but an inconvenience arises from this contrivance, which more than counterbalances its benefits. The air entering the room so near the fire immediately supplies the current up the chimney without changing the air of the room. A crowded room, and the presence of a number of lights, would, under such an arrangement, soon render the air unfit to breathe. Hence will appear the necessity for two currents into a room. The inlet for fresh air should be in a situation not liable to annoy those sitting in the room ; the outlet is generally provided for in the chimney, which is commonly sufficient for rooms of ordinary size, but is mostly too small for large public rooms.

It will be evident from what has been observed, that in order to render rooms comfortable and wholesome, two objects are required. The one is, to keep up an uniform and agreeable temperature; the other to provide for a change of the air sufficient to preserve that degree of purity essential to health, and which persons under certain pulmonary affections can so nicely appreciate.

It is evident that the former of these objects can never be attained by radiant heat; and yet, an open fire, which scarcely affords any other than radiant heat, is so connected with our domestic habits that it will be very long before the open grate will be entirely set aside. Under these circumstances, it has been found most expedient to use the combined effect of radiant heat with a constant supply of fresh air, raised to an agreeable temperature in the winter; and which, in certain cases may be cooled during the excessive heat of summer.

Great difficulties have been experienced in most of the means hitherto employed for warming air. In the first place, from what has been previously observed concerning the action of the solar rays on the earth, the air cannot be warmed by radiant heat passing through it ; therefore we can only give heat to a transparent fluid by bringing its particles in contact with a heated surface, and, in proportion as elastic fluids are more expansible, they are heated with more difficulty.

There are a number of properties which a body should possess, to afford a surface proper for heating air intended to warm and ventilate rooms. For the sake of economy it should be a good conductor of heat, in order that the radiant heat which it receives on one surface may be freely transmitted to the other. The surface to be heated should be clean, that is, free from any foreign matter, but not polished; and when the temperature can be limited, it should never, under any circumstances be allowed to exceed 300°. Metals appear to be the best substances for heating air. The temperature is limited to 300° because the animal and vegetable matter, which is found mechanically mixed with the air at all times, will be decomposed if the temperature be raised a little higher. When this decomposition takes place, as is very observable when the heated surface is red hot, certain elastic fluids and vapours are produced, which give to the air a peculiar odour, and a deleterious quality which never fails to affect the health of those who inhale it for a length of time. This oppressive sensation has been mostly felt in churches and other places where large iron stoves are used and are sometimes heated to redness. The peculiar odour accompanying it has been erroneously attributed to the iron; and on this account, earthen ware or stone has been employed to form the exterior surface of the stove. It will, however, be found that whatever be the material, if the temperature at all approaches a red heat, the same smell will be perceived; as it arises entirely from the decomposition of the matter which is in the air, and not from the heating body. This matter is very visible to the naked eye, in a sunbeam let into a dark room.

When earthen ware or stone has been employed for stoves, its inferior conducting power has seldom allowed the exterior surface to get sufficiently hot, to produce the effect on the air above alluded to. And hence it has been less objectionable as affecting the purity of the air.

It must however be admitted, that if the body used for heating the air, does not undergo any change, a metal from its being a good conductor must be preferred to any other substance. Silver or platina, if it were not for the expense, would set aside every prejudice. But long experience has shown that iron possesses every essential property. The slightly oxydated surface which is common to all iron coming from the forge or the mould in casting, is well fitted for receiving radiant heat. And if its temperature be kept below a red heat, there does not appear to be any limit to its durability. The latter point, therefore, is put out of all doubt, since it is essential, that the iron shall not be heated to a degree capable of decomposing animal and vegetable matter, in order to preserve the purity of the air which is warmed in contact with its surface.

With a view to ensure the above objects, it will be necessary to dispose of the heat as it is produced from the combustion of the fuel, in such a way, that an extensive surface of iron shall be heated uniformly without the risk of attaining a much higher temperature than 300°. This can be accomplished by making the fire of a size proportionate to the interior surface of an iron vessel, and it is found that radiant heat is much more efficacious than the heat produced by flame and conducting flues. Having heated the interior surface of an iron vessel it may be conceived that the exterior surface will quickly attain the same degree, and that whatever heat may be carried off from the exterior will be as quickly given from the interior, and instantly replaced by the radiant fire.

The next material object is the means of disposing of the heat from the exterior surface. If it be surrounded by an open space, and that be connected with a flue or tunnel of a certain height, supposing there to be no inlet at the bottom, or outlet at the top, the air will commence a circulation; that on the heated surface would ascend, and its place be as constantly supplied by the surrounding air. In this way two currents will be established; one ascending from the heated surface, and the other descending on the outside of the tunnel; and these currents will go on, as long as any difference of density exists in the air of the different parts of the surrounding space. If now an opening be made in the bottom of this tunnel and another at the top, an ascending current will be kept up; which will be as the difference of density between external air and that of the heated column, and as the square root of the height of the tunnel.

Let D be the density of the external air;

d, that in the tunnel, which will be inversely as the heat supplied.

V = the velocity which a heavy body would acquire by falling through the height of the tunnel; and $v =$ the velocity of the ascending air.

Then $v = V \times \frac{D-d}{D}$. This equally applies to chimneys, d being the density of the smoke.

The mere exposure of the heated surface in an open space, such as a small room, is not sufficient to produce the greatest effect. This is, however, the method at present used by sugar-bakers for heating the rooms in which they expose their sugars. The vessel so employed is of cast iron, and is called a cockle.

Various modifications of this method of heating air have been employed. The wall surrounding the heated vessel has been placed at various distances, in order to find the maximum of effect of a given fire. It was considered a great improvement, to place the wall at a distance, to admit of a sufficient quantity of air, and make a number of apertures in the wall, about two and a half inches square, with a view to compel the air to blow upon the heated surface. This method was employed more than thirty years ago, by William Strutt, Esq., of Derby, in his cotton-works. He afterwards made a great improvement on this plan, by inserting tubes in the apertures in the wall reaching near to the heated surface. By these means, the air is prevented from ascending before it comes in contact with the heated surface. A further improvement was made in this apparatus, by inserting similar tubes over the surface of the cockle, the shape of which was a square prism with a groined top. The cold air was made to pass through one half of the tubes; and the air so heated, became still more heated by being compelled to pass in a contrary order through the other half, into a chamber above, called the air-chamber. The stove, thus improved, has been employed by Messrs. Strutts in their works ever since, with complete success, and is similar to that by which the Derbyshire General Infirmary is warmed This stove has been fixed in different parts of the country and in London, sometimes with success; but so many circumstances besides the stove itself interfere, in arrangements of this kind, that the plan has failed in many instances. And such will ever be the case with the best inventions, in the hands of men who are unacquainted with the principles on which they are founded.

Nothing can be more obvious, than the decided advantage which this stove possesses over all others, and nothing remained for its improvement but to give its different parts their proper proportions, and to vary its construction, so as to admit of its easy management in domestic use. By the former improvement, a larger quantity of air is admitted in proportion to the fuel consumed, and of course at a lower temperature. The advantages which result from this improvement will be obvious. The ventilation of the rooms warmed by it, is much more complete from a greater quantity of air being admitted; the temperature is more uniform, from the air being more dispersed; and, lastly, from the air being heated by a greater surface at a lower temperature, the apparatus is not in the least degree injured by the fire, and hence there does not appear to be any limit to its durability.

Nothing can be more vague and uncertain, than the opinions which have been formed of the different apparatus used for warming rooms by heated air. It has in consequence appeared to me a desideratum in inquiries of this nature, to be able to ascertain the power and merits of a stove, as we do those of an engine. For this purpose, my first object was to get an instrument capable of measuring the velocity of currents. After trying a variety of methods, I have found one with which I am perfectly satisfied. It consists of a very light brass wheel, in the form of that for the first motion of a smoke-jack. An endless screw upon the same axis gives motion to a wheel of fifty teeth, on the axis of which is an index, which is watched by the eye, when the instrument is exposed to the current. The wheel acted on by the current, is about two and a half inches in diameter, and the vanes or sails are eight in number, and fill up the whole circle, when their faces are parallel to the plane of their motion, and they are adjusted to an angle of 45°. Under these circumstances, I have found that fifty revolutions of the first motion take place, while the current causing those revolutions moves through forty-six feet.

In order to ascertain the power and merits of a stove, I generally take a period of twelve hours, beginning with a good fire, and leaving off with the same. During this time, the velocity and temperature in the main warm air-flue should be taken every half hour, and then the average of each taken, keeping an account of the coal consumed in the same time. The temperature of the outer air being also known, the excess of the average temperature above the atmosphere is the datum required.

From the average velocity, the number of cubic feet of air passing through the flue in the twelve hours may be known.

Put $A =$ The number of pounds of air heated in twelve hours, allowing 14 cubic feet of air to 1 lb.

$T =$ The excess of temperature above that of the atmosphere.

$W =$ The weight in pounds of coal consumed in the same time.

$E =$ The effect of the stove, which, in stoves of all sizes on the same construction, should be generally a constant quantity: Since A the quantity, and T the excess of temperature, are advantages to be produced by W the weight of coal.

E, the effect, will be directly as A and T, and inversely as W. Therefore, $E = \frac{A\,T}{W}$.

To give an example in practice:—A stove which is capable of warming 100,000 cubic feet of space to 60° in the coldest season, when placed at the depth of nine feet below the level at which the warm air is discharged, will furnish about 45 cubic feet every second, raised 60 degrees above the temperature of the atmosphere. To keep up this current and excess of temperature for twelve hours, it will consume not more than three bushels of coals, or 252 lbs. In this case, 49 cubic feet of air in each second will be 1,944,000 in twelve hours, equal to 138,857 lbs. Hence $E = \frac{138{,}857 \times 60}{252} = 32{,}930$. This number may be taken as a constant quantity, expressive of the power of any stove; but it also expresses the weight of air in pounds, which one pound of Newcastle coal heats one degree of Fahrenheit's thermometer.

This number will not be strictly a constant quantity, as small stoves will not act quite to the same advantage as larger ones; and local and other circumstances will in some degree alter the result of experiments made in the manner above stated. This is more especially the case, when the admission of cold air and the discharge of foul air, are in any degree influenced by the wind.

The cold air is generally brought directly from the atmosphere; and, therefore, as its progress along this channel is affected by the wind, a greater or less quantity will pass through the stove. If the air be deficient, less heat is carried off from the heating surface, and a greater proportion goes up the chimney; on the contrary, when the wind blows into the cold air-flue, the two forces conspire, more air is admitted, more heat is carried off with the air, and of course less is wasted up the smoke-flue.

In all situations where it is practicable, I use an effectual means of regulating the admission of cold and the escape of foul air, by placing at the commencement and termination of these apertures a turn-cap or cowl, in which the vanes are so fixed as to let the wind blow into the one, and assist the escape of air from the other. Although this contrivance will always prevent a counter current, which without its use is sometimes the case; it does not prevent unequal quantities of air from entering, according to the strength of the wind. This is not found in practice to be a great inconvenience; for during the most perfect calm, the air admitted by the power of the stove alone, is sufficient for every purpose of warmth and ventilation: whilst with a tolerable fire in the stove when the wind is considerable, the air comes into the rooms at a higher temperature than the rooms require which is at least erring on the desirable side. If the quantity of air admitted under all states of the wind were required to be uniform, the aperture in the turn-cap for cold air might contain a self-adjustment, by the action of which its area would always be in the inverse ratio of the velocity of the wind; by which means equal quantities of air would always be admitted in equal times.

The turn-cap for the escape of foul air is placed at the top of the building, and is made common to the roof. Under this arrangement all the rooms into which the warm air is admitted have each a foul air flue terminating in the cavity of the roof.

The contents of all the foul-air flues are therefore ultimately discharged at the turn-cap. This arrangement is adopted at the Derbyshire General Infirmary, and at the Wakefield Lunatic Asylum. In the summer season, when the stove is not in action, the ventilation will depend on the wind, which at some periods may not be adequate to that change of air required in hospitals. In such cases I have adopted an additional means of ventilation. Instead of making the foul-air turn-cap common to the roof, I have placed it at the top of a cylindrical cavity built in the roof. Into this cavity I bring all the foul-air flues, which also in this case may be smoke-flues, if constructed with brick. I also connect with the same cavity, the stove chimney, and, if possible all the other smoke-flues in the building. By this means, it may be expected, that some degree of rarefaction in the cylindrical cavity in the roof will be constantly going on, and that hence a perpetual current will be established from every room towards the general outlet. It would be difficult to adapt such an arrangement to old buildings, without great alteration in the roof. But it would be easily introduced into new houses. The advantages derived from it in ordinary dwellings would be very great. In the first place, there could not be an instance of a smoky chimney; in the next, a down current in an unoccupied chimney could not occur, and therefore the passage of the smoke of one chimney down another would always be prevented; and lastly, by having only one outlet for smoke in every house, and that an object which may be made ornamental, we should ultimately get rid of the great deformity which arises from the present appearance of chimneys in buildings.

In all situations where it is practicable to make a cold air flue, of considerable length under ground, the advantage is well worth securing. I have found by experience that a cold air-flue of 50 yards in length is capable of cooling the air in summer to about an arithmetical mean between the temperature of the air and the earth, and a similar advantage is produced by the earth warming the air in the winter season. The shape of the cold air-flue should be such as to present the greatest possible surface; the very contrary being essential to the best construction of flues for the warm air.

These facts will successfully lead to the means of cooling buildings in the tropical climates, and of warming the air when the winter's cold is much below the temperature of the earth.

Great Russel-street, Bloomsbury, May, 1821.

ART. II. *On the Height of the Dhawalagiri, the* White Mountain of Himálaya. *By* H. T. COLEBROOKE, Esq.

[Communicated by the Author.]

IN an essay on the height of the *Himálaya* Mountains, which was inserted in the 12th volume of *Researches of the Asiatic Society,* I offered reasons for the opinion of their great elevation: relying especially on the measurement of the *White Mountain,* emphatically so named, which towers above the rest of the snowy peaks seen from the plains of *Hindustan.* Its height was computed, from three sets of observations, taken by Captain Webb, at 27,551 feet above the observer's stations in *Gorakhpúr;* or 27,677 feet, making that allowance for refraction, which was found to bring the result of the several observations nearest to agreement. Even assuming all errors to be on one side, and in the extreme, it would appear to be 26,862 feet at the lowest computation. But such extremity of errors is hardly presumable; and considering the supposition of compensation of errors, and ordinary rather than extraordinary refraction, to be more likely correct, the inference was that the *White Mountain* may be about 27,600 feet above *Gorakhpúr,* or nearly 28,000 feet above the level of the sea.

Arguments were likewise deduced from observations of Colonel R. Hyde Colebrooke and Colonel Crawford, for the altitude of other lofty peaks; many exceeding 22,000 feet, and some rising to 24,000, and even 25,000 feet.

These calculations and estimates of elevation were exhibited as a mere approach towards a determination of the true height, yet substantiating the general position that the *Himálaya* is the loftiest known range of mountains; its most elevated peaks greatly exceeding the highest of the Andes.

That position, as well as the approximated determination of heights on which it was grounded, has been controverted. But the course of political events having since afforded facility of access which was before denied by the jealousy of the *Gurkháli* Mountaineers, accurate measurements, both barometric and trigonometric, of a great number of points, in the vicinity of the upper *Ganges*, *Jumna*, and *Setlej* rivers, have been carefully taken by different Surveyors, which irrefragably establish the general position of the transcendent altitude of the *Himálaya*: and a great multitude of peaks have been determined, which exceed 22,000 feet; a few rising above 23,000; and one measured by Captain Webb, no less than 25,669 feet.

These however do not equal the stupendous altitude of *Dhawalagiri*, or the *White Mountain;* also named *Gásákótí*. The routes of travels and surveys have not hitherto been directed to its vicinity. Their direction has been towards the upper Ganges and the *Setlej*. The more easterly mountains, toward the sources of the great *Gandhac*, have not been approached; and the measurement of the *White Mountain*, taken from the plains of *Gorakhpúr*, is yet to be confirmed by observations from nearer stations.

Previously, however, to the occurrence of those events, which have been alluded to as having laid open the mountainous confines of Hindustan to research, observations had been again made in the plains of *Gorakhpúr* to determine the elevation of *Dhawalagiri*, on the same principle on which Captain Webb proceeded during his previous survey in that province. Captain Blake, to whose labours these further observations are due, has been so good as to communicate to me the particulars of them: I subjoin his letter.

The height of *Dhawalagiri* and four contiguous mountains has been computed from the data furnished by Captain Blake's survey; using the formula which I gave in the 12th volume of *Asiatic Researches;* and taking terrestrial refraction at one eleventh of the contained arc, which is the estimated mean quantity of ordinary refraction. The elements of the computation are exhibited in a tabular form accompanying.

The elevation of *Dhawalagiri*, taking the mean of three sets of observations, is thus found to be 27,615 feet above the plains of *Górakhpúr*, or 28,015 above the sea; differing only 64 feet from the computation founded on Captain Webb's survey.

Or allowing for terrestrial refraction one twelfth of the base; which was the rate that appeared to bring the result of the different observations taken by Captain Webb, nearest to agreement; the elevation of *Dhawalagiri*, deducible from the later survey, is 27,704 feet above *Gorakhpúr*, or 28,104 above the sea; differing from that deduced from the former survey by no more than 27 feet.

This near coincidence authorizes an expectation, that the true height of *Dhawalagiri*, when it shall be accurately determined, will be found very little wide of 28,000 feet.

Captain Blake's survey determines likewise the positions and altitudes of four other conspicuous mountains in the vicinity of *Dhawalagiri*.

About thirty-six miles east of it, and equally distant from the plain, is a mountain which rises to the height of 23,708 feet above *Górakhpúr*, or 24,108 feet above the sea.

Nearly midway between them, but somewhat less remote, is situated a mountain called *Set-ghar** or *Nepal*; its position, as the last mentioned name implies, is near to *Nepal* proper. The elevation of the summit is 24,861 feet above the level of *Górakhpúr*, or 25,261 feet above that of the sea.

Twenty miles west of *Dhawalagiri*, but less remote from the plains, are two peaks, rising from the same mountain mass, for they are but four and a half miles asunder. The highest of the two is *Chandragiri,* or Mountain of the Moon, a name common to many others. Its elevation is 22,607 feet; that of the contiguous lower peak is 21,535 feet above *Górakhpúr;* or 23,007 feet and 21,935 feet respectively, above the sea.

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* Probably *Swétaghar*, or the White Tower.

These altitudes, though much short of *Dhawalagiri,* tend to corroborate the estimate of its great elevation: for it may be seen in Captain Blake's sketch of the appearance of this portion of the snowy range, how greatly *Dhawalagiri* overtops the rest, lofty as they are.

It is to be hoped that some traveller may be induced to visit the *Himálaya* in that quarter, and explore the great *Gandhakí* river to its source at the foot of *Dhawalagiri,* and determine the elevation of the mountain by observations at the nearest accessible heights. Besides other subjects of research, it presents one of much interest in the abundance of organic remains there found: for it is thence that devout *Hindus* are supplied with Ammonites, an object of their idolatrous worship, under the appellation of *Sálagráma.*

H. T. C.

To H. T. Colebrooke, *Esq. &c. &c. &c.*

DEAR SIR,

Having been appointed, by the Government of Bengal, in the year 1812, to survey the extensive province of *Gorakhpúr;* that year, the whole of 1813, and part of 1814, were occupied in surveying the southern portion, or that division of the province, lying south of the *Gograh,* or Great *Saryú* River. Proceeding northward, at the subsidence of the rains, in 1814, I had on the 1st of November near the village of *Urwára* a distinct view of the snowy summits of the *Himálaya* mountains; and from this station I took, with one of Troughton's theodolites (of six inches radius), the bearings and elevations of five of the snowy peaks, being those that were most remarkable; of which three possess names, and two are anonymous: the former are well known to the inhabitants of the subjacent champaign (situate southwest of the mountains from 60 to 140 miles) under the appellation of *Chandragiri*; *Dhawalagiri*, (or Gasah Kotee); and *Sétgar*, (or Nypal.) At this station, although distant nearly one hundred and forty miles, these mountains have on the whole the most sublime aspect: this may be attributed to the smaller range of dark hills being lost at that distance in the perspective, for upon a subsequent and nearer view, at the station of *Maha deva diuriya*, the smaller hills intercept a refracted portion of the great chain; I say refracted, for a right line drawn from the first station to the base of the snowy peaks, would (owing to the spherical figure of the earth) at that distance, pass far below the base of the smaller range. From this first station the bearings of the five snowy peaks (corrected for magnetic variation of 2° 13′ 39″ East) are as follows, viz.

Peak A. bearing N. 8° 23′ 39″ E., and elevation 1° 5′
B, or *Chandragiri*, N. 10° 5′ 39′ E., and elevation 1° 7′
C, or *Dhawalagiri*, N. 17° 13′ 39″ E., and elevation 1° 18′
D, or *Seitgur* (Nypal.) N. 26° 13′ 39″ E., and elevation 1° 15′
E, bearing N. 34° 43′ 39″ E., and elevation 1° 6′.

From station No. 1, I proceeded to the north side of the town of *Bánsí*, which I shall call station No. 2, the latter station bearing from the former (allowing for magnetic variation) N. 14° 30′ E., and distant as inferred from survey (protracted on a scale of two miles to the inch) $25\frac{3}{10}$ British statute miles. From hence the bearings (allowance being made for magnetic variation as before) and elevations are as follows, viz.:

A, bearing N. 6° 46′ 39″ E., and elevation 1° 41′ 15″
B, N. 9° 3′ 39″ E., and elevation 1° 45′
C, N. 13° 1′ 39″ E., and elevation 1° 56′ 30″
D, 29° 3′ 39″ and elevation 1° 53′
E. N. 36° 47′ 39″ E., and elevation 1° 33′.

At this station (No. 2,) on the 3rd November at day-break, the *Himálaya* displayed an exceeding white appearance, and the sun's rays passing through a red cumulo-stratus cloud, coloured the snowy peaks on their prominent parts only, with a most beautiful pink tint. This took place a long time before the sun rose with us.

The first aggressions of the *Goorkahs* commenced in this province, by murdering some of the police, a T'hanadar and two or three others, who had been but a short time previous to the catastrophe in attendance, with me, to point out the boundaries of their respective districts. The *Goorkahs* were at this period posted near to our route, in defiance of the division of the army, which had been some time assembled under the command of General Sulivan Wood at *Gorrukpoor;* the alarm created among my people, owing to our vicinity to the enemy, was so great, that it was scarcely conquerable by the utmost persuasion. A curious instance of the audacity, as well as superstition of the enemy was current here, said to have occurred in this neighbourhood within a month. A part of the *Goorkah* force came to the banks of the *Rápti* river (which runs close by this station) and sacrificed a pig, as a propitiatory offering for success in the existing war; and on relating it to Colonel Fagan the adjutant-general of the army, he informed me, that upon the determination of the *Goorkahs* to war with us, they, horrid to relate, sacrificed in the mountains, a human being, as an offering for success to their arms.

From the station No. 2, still proceeding northward, I arrived at station No. 3, (situate near to the village of Mahá-Déva-Diúriya,) bearing from No. 2 (corrected for magnetic variation), N. 8° E., and distant from the same (as inferred from survey) 16 British statute miles. From hence the snowy peaks bear and elevate as follows, viz.:

A.	N. 6° 48′ 39″ E.	elevation 2° 13′ 30″
B.	N. 9° 22′ 39″ E.	elevation 2° 16′
C.	N. 20° 47′ 39″ E.	elevation 2° 32′
D.	N. 32° 32′ 39″ E.	elevation 2° 26′
E.	N. 41° 16′ 39″ E.	elevation 2° 1′.

At this station was distinctly heard the evening and morning guns of the *Goorkah* forces in the neighbourhood; and the alarm created among my people increasing, induced me to bend my course westward, for a base line parallel to the mountains, to ascertain their horizontal distance; which, in three days, brought us to the village of *Biskúr*, or station No. 4, bearing from station No. 3, (corrected for variation as before), S 77° 46′ 21″ W., and distant $24\frac{5}{8}$ British statute miles. From this station No. 4, the snowy peaks, corrected for variation, bear as follows:

A.	N. 20° 43′ 39″ E.
B.	N. 23° 5′ 39″ E.
C.	N. 31° 55′ 39″ E.
D.	N. 42° 26′ 39″ E.
And E.	N. 49° 13′ 39″ E.

The distances of the aforementioned five peaks resulting from trigonometrical measurements, (and contained in a former paper, which I had the honour of presenting to you,) approximate to those distances, which result from the intersection of rhumb lines of the bearings of the peaks from the stations Nos. 3 and 4.

I remain, &c.

B. Blake.

Computation of the Height of Dhawalagiri and other Mountains.

MOUNTAIN.	Station.	DISTANCE.		Contained arc.				Altitude observed.				Corrected for refraction.				Angle subtended.				Elevation inferred.	MEAN.
		miles.	feet.	°	′	″	‴	°	′	″	‴	°	′	″	‴	°	′	″	‴	feet.	
A.	1	126¾	669240	1	50	26		1	5			0	55			1	50	21		21520	21535
	2	101½	535920	1	28	26		1	41	15		1	33	13		2	17	26		21449	
	3	85¾	452760	1	14	42		2	13	30		2	6	42		2	44	3		21635	
Chandragiri .	1	129⅛	681780	1	52	28		1	7			0	56	46	33	1	53	0	33	22435	22607
	2	103⅞	548460	1	30	30		1	45			1	36	46	20	2	22	1	35	22686	
	3	88⅛	465300	1	17			2	16			2	9			2	47	30		22699	
Dhawalagiri, or Ghasa-cótí	1	139	733920	2	1	0	40	1	18			1	7			2	7	30	16	27255	27615
	2	113¾	600600	1	39	7		1	56	30		1	47	29		2	37	2	30	27430	
	3	98	517440	1	25	23	10	2	32			2	24	14	15	3	6	55	50	28161	
Swetaghar, or Nepal . .	1	131¼	693000	1	54	21		1	15			1	4	36	16	2	1	46	46	24576	24861
	2	106¾	563640	1	33			1	53			1	44	32	44	2	31	2	44	24785	
	3	91¾	484440	1	19	57		2	26			2	19	20	15	2	59	18	45	25223	
E.	1	136⅝	721380	1	59			1	6			0	55	11		1	54	41		24092	23708
	2	113½	599280	1	8	53	30	1	33			1	24	0	35	2	13	27	20	23292	
	3	99	522720	1	26	15		2	1			1	53	9	33	2	36	17	3	23740	

ART. III. *Remarks on Marine Luminous Animals.* By J. MAC CULLOCH, M.D., F.R.S., *&c.*

[Communicated by the Author.]

IN my work on the Western Islands of Scotland, I had occasion to take notice of the causes which produce that beautiful appearance of light in sea water, so well known to seamen, and to all indeed who have been in the least conversant with the sea during the darkness of night. I there attempted to prove, that if, in every case it did not arise from the action and properties of living animals, but was sometimes owing to the luminous matter of fish dispersed through the water, yet that all the most conspicuous appearances of this nature were produced by these, and that the brilliant sparks of light, in particular, were always to be traced to some of the vermes or insects, which abound in the waters of the sea.

I have also given a list of such of these animals as had, by various naturalists, been found to possess this remarkable property; and had occasion to lament how circumscribed it was; partly owing to the deficiency of observers in this department of Natural History, and partly owing to unfounded theories respecting the nature and causes of the light of the ocean; in consequence of which, those who possessed the opportunities of extending this examination, had neglected it. I have also observed that many animals either very minute, or absolutely microscopic, and invisible without the use of a lens, existed in the sea; and that the neglect of these more obscure creatures, had probably been one reason why the property of emitting light was referred to the water itself, when it was, in fact, owing to these unsuspected animals existing in it.

The further investigation of this department of Natural History, was, in that essay, recommended to those who might have opportunities of pursuing it, as the subject had not at that time practically engaged much of my time, being occupied by geological pursuits requiring undivided attention, and every leisure moment of the night being employed in registering the observations of the day. But as it is not often that observers feel much interest in pursuing a track which has been laid down by their predecessors, unless perhaps for the purpose of controverting or disputing the principles or facts on which it has been grounded, I thought it right to make use of such further opportunities as might occur towards the accumulation of new matter on this subject, and towards confirming the opinions stated in the paper to which I have alluded. A voyage to the Shetland and Orkney Islands afforded these opportunities; and the result has been to confirm the former views, by a series of observations carried on daily for many weeks. By these a large addition has been made to the list of luminous animals which was given in that essay; and it has been, in particular, proved, that the sea is very often crowded with worms and insects, often nearly invisible; and that the luminous property of the water, not only bears a relation to the existence and numbers of these at any time, but may almost always be traced to the individuals by which it is caused.

Those who are acquainted with this obscure and much neglected department of Natural History, will not be surprised to hear that I cannot at present give names to the numerous individuals which I examined for this purpose. Among them are many objects, of which, not only the names are doubtful, but the very genera, and even the analogies, obscure or uncertain. Many are absolutely unknown, and constitute new species which it will be my business to describe hereafter, when all the requisite comparisons have been made. For the present purpose, it is as unnecessary, as it would be impossible, to enter into details of so extensive a nature as would be required for assigning the names of the various animals in which I have now observed the property of emitting light, in addition to that list which was given in the essay to which I have here referred.

It will not be useless to those who may be inclined to pursue the same train of investigation, to describe the means which I adopted for examining the animals in question; while it will further the purpose of explaining the species of evidence by which I was satisfied respecting the nature of the objects which were examined, and more particularly respecting their powers of yielding light. If there is any deficiency in the nature of this proof, as it relates to some of the more minute animals, there will still remain a considerable number to add to the list formerly given.

It must in the first place be remarked, that the whole of these observations were confined to spaces in the sea never extending above 8 or 10 miles from land; and that they were very generally made in harbours. They cannot in fact be made at sea; at least in a small ship, unless it is smooth water: as the agitation of the water under examination, no less than that of the observer's person, renders it absolutely impossible to catch and detain the objects before a lens in such a manner as to examine or delineate them. At all times, even in harbour, it is sufficiently difficult from the motions of the animals themselves, to obtain such views of them as to satisfy ourselves respecting the nature and characters of those which are minute, and of which the greater number are exceedingly restless and rapid in their movements.

Although a great many of the animals which fell under my notice, were found at the distances from land which I have just mentioned, many were only discovered in harbours, and, nearly at all times they were far more abundant in these situations than in the open sea. Some of them, it is true, seem to disregard boisterous weather; but there were many which almost invariably disappeared on the coming in of a fresh gale, and only re-appeared when the weather moderated. Other changes of weather or wind, often caused them in the same manner to disappear in the course of a few hours. It is probable that these animals, like the leech, are very sensible to atmospheric changes, and that they retire to deeper water to avoid that agitation, which, to many of the larger, would be fatal, from the tenderness of their texture and from their bulk. Many are probably destroyed by the violence of the sea at the surface. These are hints which may be of use to any naturalist inclined to enter on this department of his pursuit, while they assist in explaining the variations to which the luminous property of the ocean is subject; and the addition of a few more will not be misplaced to those who have not had any experience in these investigations.

These animals always abound most, with few exceptions, in the smallest harbours, and, more particularly, in narrow creeks, among rocks or under high cliffs, where the water is sheltered from the sea and wind, and where it is consequently seldom so much disturbed as in more open places. A large proportion of them indeed seems to be exclusively limited to situations of this nature, being never found in the open sea nor far from shore. Many of the minute marine animals also appear to affect exclusively those shallow and rocky situations where sea weeds abound, and which are equally the favourite haunts of many larger species, such as nearly the whole tribe of crabs, and many others which it is unnecessary to enumerate.

It is in such places then, and at such times, that is, in narrow and rocky creeks or weedy shoals, and in calm weather, that the naturalist will meet with most success; and it is in such circumstances also that the water will be found most luminous.

That it does not always appear luminous in calm weather, and when the vessel is quiet at anchor, is however certain; and it is this which has conduced to mislead observers respecting the causes of the light, as well as to lay the foundation of fallacious prognostics regarding the weather. It requires agitation to elicit the light of these animals in abundance; and as this naturally happens in troubled water, they have been supposed to abound in gales of wind and in a breaking sea, when they are, in fact, comparatively scarce. In calm weather, crowds of medusæ or other very luminous species, will often be floating around, yet betraying themselves only by an occasional twinkle; when any disturbance communicated to the water is sufficient to involve the whole in a blaze of light.

I formerly remarked, that the luminous action was voluntary; and this opinion has been amply confirmed by further attention to the animals possessed of this property. Among millions of these, of numerous species, the usual actions of locomotion will be performed for hours, or for a whole night, without the slightest indication of their presence; or perhaps some individual will give an occasional spark as it passes by, when the dipping of an oar, or the drawing of a bucket of water, is sufficient to render the whole around luminous.

It is by such a test as this, therefore, that a naturalist will be guided in his pursuit after these animals. But it is proper to remark that it is often very difficult to take them, even when we are certain that they abound in the water; and this cause, like others, has often made it to be supposed that the water itself possessed a luminous property, because no animals appeared in a bucket when filled with it. A few bright lights produce a considerable effect in the night, so as to make the sea appear much fuller of sparks than it really is; and it is easy for a body so small as the ship's bucket to miss the animals by which they are produced. Moreover, as many of these creatures, and particularly the medusæ, swim near to the surface, they are apt to slip out with the wave which is produced by lifting the bucket out of the water; so that it sometimes requires many attempts before one can be secured.

There is another circumstance which is also an occasional source of error respecting the existence of these animals in the water when brought up; even when it is highly luminous alongside the vessel. Whether from fatigue, or from caprice, or from some voluntary efforts for an unknown purpose, they often refuse to show their light, even when violently agitated or injured; and, in all cases, when they have been compelled to shew it for a few seconds by violence, they again become dark and refuse to shine any longer. It is not unlikely that this is the effect of exhaustion; because after a repose of some little time, a fresh disturbance often causes them to give light again. A naturalist, unaware of this circumstance, may often imagine that he has failed in procuring specimens, even when the bucket is crowded with them.

Another circumstance leads to deceptions in these cases. In many of the luminous worms and insects, the spot of light appears much larger, if it is not really so, than the body of the animal; and very often a species which is invisible under ordinary circumstances, or only to be seen by bringing it opposite to a bright light in a glass of water, will yield a very brilliant and large spark. Thus, in a ship's bucket, or a basin, it would not be conjectured that any animal existed, when many thousands are present; and, of these, perhaps the greater number, if not all, highly luminous.

It is, lastly, necessary to remark, respecting the size of these animals, as just mentioned, that many of the luminous species are absolutely, and under all circumstances, except when in the act of emitting light, invisible to the naked eye. This effect arises in some measure from the actual minuteness of many, their size not equalling the 100th of an inch; but in many others which subtend a visible angle, it proceeds from their transparency. Even under favourable circumstances, as when placed in a glass of water, where the vision is aided by the magnifying power of this species of lens, they cannot easily be discovered; owing to the water in which they abound being invariably muddy. Those only come into view which approach so near to the fore part of the glass as materially to diminish the column of water between them and the eye; and thus also they often escape observation, and the spectator is surprised to find that he can discover nothing in the light, when the water, in the dark, has abounded in luminous sparks. If the lens is used, it is still only in the observer's power to get sight of those which pass across its focus; so that he is, in this case also, apt to underrate their numbers, or, if rare, to doubt their existence. It is fruitless to attempt to bring them under the eye by using a small drop of water in the manner adopted in microscopic observations; as, even where most crowded, they bear so small a proportion to the water in which they swim, that such a drop may not possibly contain one.

These then are the most important circumstances which the naturalist should have in view in investigating the water of the sea for the purpose of discovering the minute animals which exist in it; whether for the purpose of ascertaining their luminous quality, or of examining their nature and structure. An attention to these cautions will probably assist others, as it did myself in these examinations; and induce them to believe what seems to me fully ascertained, namely, that luminous animals abound in the water of the ocean even when they are least suspected, and that the property of emitting light is probably granted to every one of these neglected inhabitants of the deep.

When the numbers of these animals are considered, it will appear less extraordinary that the water of the sea should be so generally luminous; and, when we attend to their minuteness, it is as little cause of surprise that they should escape ordinary observation. Having necessarily reserved the description and names of the species for future communications, partly for the reasons already stated, and partly because they could not be rendered intelligible without drawings; I shall not enter on this part of the subject, but merely attempt to convey an idea of the numbers of some of the most remarkable individuals which were examined.

In proceeding from the Mull of Cantyre to Shetland, with beating winds nearly the whole way, it is easy to understand that an immense tract of water must have been passed over. Those whose memory can so easily refer to the map of Scotland need not be told of the number of square miles which a vessel must traverse in this navigation. With very little exception throughout all this space, and in every one of the harbours of Shetland and Orkney, the water was full of one species, in particular, of an animal which I think is not yet described. It scarcely ever quitted the vessel, although more abundant in some seas than in others. On a very moderate computation a cubic inch did not contain less than an hundred individuals; and as they were brought up from all depths to which the bucket could be sent, it is useless to attempt a statement even of those which must have been contained in a few cubic feet, much less in the enormous mass of water thus examined. Their numbers, even in a superficial mile, supposing its depth not to exceed a few inches, baffles all imagination. This species was barely visible by the naked eye, when viewed in a glass against the light of the candle or of a moderated sunbeam.

In the same seas, and nearly at all times, the water was found filled with several different species, resembling in size some of the infusoria, and invisible without the lens. To estimate their numbers is equally impossible, but no body of water so small could be brought into a proper situation without being found filled with them. Other animals of larger dimensions, and of many species were equally constant; and, if less numerous, yet ten or twenty were always to be found within the space of a common tumbler glass.

In all these cases the water was luminous; and, that it was rendered luminous by these animals, admitted of no doubt, because the larger individuals could be taken out on a dry body, shining at the very moment of their removal, and then replaced for examination in water: while the light of the whole of these species disappeared when they died, either from keeping the water too long, from warming it, or from the addition of spirits. The facility with which the luminous quality of sea water is destroyed by those means which kill its inhabitants, is in itself a sufficient proof that the cause of this property resides in these.

I must further add, that it is perfectly easy to distinguish the different sparks of light given by different animals; that is, as far as they differ in dimensions; as the bright spot is quite distinct in the larger kinds, in which it also often varies in colour; while, in the smaller, agitation produces a general luminous appearance, in which separate spots, or the distinct action of individuals, is not to be recognised; it is probably therefore rather from this source, namely, the crowd of microscopic worms and insects, that the general luminous track produced by a fishing line, or the faint sheet of light elicited by the dash of an oar, is caused, than by the detached secretions of fishes, or by decomposing animal matter diffused through the water; while the brighter separate sparks arise from the larger kinds, to the size of which they are more or less proportioned. It will in the same way, be found, that the predominance of bright sparks in the vicinity of sea weed, or near rocks, arises from the great number of species, Squillæ, Scolopendræ, Nereides, and many others, which make these places their exclusive residence.

It is now necessary to point out the method used in examining these animals, and deciding on their luminous powers.

With respect to the larger kinds, there is no difficulty; the smaller require many more trials; and where more than one species persist in occurring together, some uncertainty must always remain. Yet where a property is, in so many instances, ascertained to exist, and where it has probably been conferred for the specific purposes formerly noticed in the essay to which this communication must be considered as an appendix, it is not a rash conclusion to consider that no species is exempt from the general law or deprived of this power; since in the most essential circumstances, the habits of all are the same.

These animals, whether the smaller vermes or insects, are very rarely found in clear water, and wherever they are abundant it is muddy, or rather fouled with some animal matter which communicates to it a slight milky hue; although they are not, on the contrary, necessarily present when the water is in that state. It is preferable to examine the water by candle-light, as ordinary day-light is not sufficient for the purpose; and the light of the sun cannot easily be received in such a manner as to be endured by the eye, and, at the same time, to serve the purpose of illuminating the objects. It is desirable to use more than one candle, as it is convenient to have more than one luminous spot under command; the rapidity of the motions of most of these animals, carrying them so quickly beyond the limits of one spot, as to cause considerable trouble to the observer, who has many things to distract his attention at the same time. Some of them are better examined in the brightest light; others at its borders; and, very often, it is necessary to examine the same object in different lights before a just idea of its form can be obtained. A separate light is also required to illuminate the paper on which they are to be drawn; the eye being so far paralyzed by the excess of light required to view them, as not to see in a moderate degree of illumination, and it being absolutely necessary to draw them, without losing the least practicable interval of time after viewing them through the lens. A few seconds are sufficient to cause the observer to forget the exact figure of the parts which he is to delineate.

The most convenient receptacle in which they can be placed for examination is a rummer or conoidal glass, of such dimensions as to contain half a pint. It is, in the first place, quite necessary that they should be at liberty: as it is only when in motion that many of them can at all be discovered, and as the peculiar nature of their motions, which, in all, are very different and highly characteristic, is of great use in discriminating individuals otherwise much resembling each other. It is true, that this is productive of great inconvenience, from their passing so quickly out of the field of view; and thus it often requires a long time and patiently repeated examinations, to ascertain the exact figure of one individual. But it is impossible to confine them in a drop of water, unless when absolutely microscopic, without losing sight of their forms. In this case, they come to a state of rest; and their fins, legs, antennæ, or other fine parts, become invisible, generally collapsing close to the body. Moreover the affection of light produced by the contact of the animal with the surface or edge of the drop, or of that of the drop with the glass on which it stands, totally destroys distinct vision, and renders their forms quite unintelligible. A glass of smaller dimensions, such as a wine glass, is far less convenient than that abovementioned; as the smallness of the convexity produces a much less useful spot of light.

In many cases, where, from excessive activity, it is difficult to catch these objects in the field of view for a sufficient time to study their parts, I have found it useful to diminish their powers of motion. This may be done by slightly warming the water, by suffering it to stand for a few hours in the glass, or by the addition of a small quantity of spirits, and probably of other substances. But slight injuries are sufficient to kill them; and, as they then become invisible, the observer must be on his guard not to exceed in the application of these means.

From the necessity of using a large glass, and the freedom of motion thence allowed, it is evident that a high magnifying power cannot be applied. It is scarcely possible indeed to make effective use of one greater than that produced by a simple lens of half an inch focal distance; and as, with this power the field of view is very contracted, it is often convenient to have two other lenses at hand of one inch and of two inches in focal distance. The very minute ones may be occasionally secured in a single drop of water under a compound microscope; but the observer will be disappointed much oftener than he will succeed in his attempts to examine them in this way; partly from the chance of his failing to find any in many successive small portions of water thus separated, and partly for the reasons just stated.

I have already mentioned almost all that occurs on the method used in determining those species which were luminous. Of the larger kinds, it seldom happened that more than two or three, sometimes not more than one, was contained in a tumbler. Being placed in the dark, and stirred with the finger, the same number of sparks were produced; and whatever failure might here have occurred in one trial, was removed by others made at different times. With regard to the smaller species, it sometimes happened that only one was found on a particular occasion, and the luminous state of that water on agitation proved the property to exist in that individual species. Respecting some of these species, however, doubts may remain; as in some cases no one of them was found alone. But these doubts are of little consequence; since if among so many animals resembling each other in their general characters, and often indeed apparently belonging to the same genus, the luminous property was certainly proved to exist in some, it probably existed equally in all; as there seems no reason to exclude any, or to suppose it especially possessed by one. On this subject, however, other naturalists must be allowed to judge for themselves; and those who are inclined to pursue the same train of investigation will probably complete the evidence respecting some where it is here left doubtful.

I may now therefore conclude this subject by remarking, that, from the investigation of last summer, I have added upwards of 190 species to the list of luminous marine animals. I have already stated the reasons why I cannot as yet give even the names of many of these, of which a considerable number are certainly new, or nondescript animals. That subject must be reserved for a different species of communication; but I shall here add at least the generic names of those possessed of luminous properties, of which the genera are known; since, even in these, some of the species are still unsettled and many are new.

Among these, the most conspicuous are about twenty small species of Medusa, in addition to those already known to be luminous. In the ancient genus Cancer, a considerable number of Squillæ were also found possessed of this property. In the genera Scolopendra and Nereis five or six were luminous, being all the species that came under my observation. Of the remaining known genera in which luminous species were observed, I shall forbear to give any numerical account, but simply add that they consisted of Phalangium, Monoculus, Oniscus, Iulus, Vorticella, Cercaria, Vibrio, Volvox. To these I may also add, among the fishes, a new species of Leptocephalus. The rest consisted of new genera, or, at least, of animals which, for want of correct descriptions and of figures, cannot be referred to any as yet to be found in authors, and of which I trust at some future period to give those drawings and descriptions which are in my possession. It is sufficient for the present purpose to have shewn that the list of luminous animals is very extensive, and to have given this notice of the means used in investigating this object, together with such hints as may be useful to others; little doubting that their labours will ultimately prove this beautiful and remarkable property to be possessed by every one of the inhabitants of the ocean.

But I must not conclude this paper without noticing a circumstance which confirms the opinion stated in the former essay respecting the residence of many fish in depths which, according to Mr. Bouguer's observations, must be supposed inaccessible to the light of the sun; and in which, without that afforded by their prey, it is difficult to understand how they can find their food. It is remarked by the Shetland fishermen, that the ling invariably inhabits the deep valleys of the sea; whereas the cod is always found on the hills, general known by the name of banks. In one of the most productive spots for the ling fishery, the valley which they inhabit is not only very deep, but is bounded by abrupt land or submarine hills nearly precipitous; the water suddenly deepening from 20 and 30 to 200 fathoms. In this, as well as in other valleys in which this fishery is carried on, always very far from the shore, it is found that the best fishing exists at the greatest depths, and it is not unusual to sink the long lines in water of 250 fathoms depth. The time required in setting and in drawing up from this depth, the length of line used, which amounts in some cases even to seven miles, is such as to prevent the fishermen from making any attempts in deeper water; but they are all of opinion that this fish abounds most in the deepest places, and might advantageously be fished for at much greater depths. Now allowing even 1000 feet instead of Mr. Bouguer's calculation of 723, it is plain that no light can exist in these valleys, and that the ling, like other fish which prey in the deep seas, must have some means of seeing his food, as well as of pursuing his social avocations of whatever nature these may be. This can only be effected by the luminous property, either of his prey, or of the animals which abound in the sea, or else by that elicited from his own body.

J. MAC CULLOCH.

Shetland, August, 1820.

ART. IV. *A Translation of* REY'S *Essays on the Calcination of Metals*, &c.

[Communicated by JOHN GEORGE CHILDREN, Esq., F.R.S., *&c.*]

Continued from Page 83.

ESSAY IV.

Air and Fire have weight, and naturally descend.

HAD we as free a commerce with the elements of fire, as we have with the air, we should doubtless, be furnished with experiments, to confirm our assertion. True it is, that those which we shall produce with regard to the latter, will be conclusive as to the former, from the proximity of their nature*. Now since it is agreed, that whatever falls downwards of its own accord, has weight, whence that motion proceeds, who is he, that shall deny this quality to air, seeing that we no sooner pull a stake out of the ground, than the air rushes into the hole, and fills it; and that we cannot dig a well so deep, that it does not immediately descend into it, without any external effort or violence? I say more: that, if there were a tube from the centre of the earth to the region of fire, open at both ends, and filled with the four elements, each in its usual position, if the earth were drawn downwards, water would descend and occupy its place, leaving its own to the air, and the air its place to fire. Then, taking away the water from this station, the air will come and fill it, and that again being taken away, fire will go into it, and fill the whole tube, descending to the very centre, merely by being deprived of that, which prevented it from doing so. They who shall say, that this happens, that a vacuum may be avoided, will not say much; they will shew us the final cause, whilst we are talking about the efficient, which cannot be a vacuum. For it is quite certain, that in the boundaries of nature, a vacuum, which is nothing, can have no place. There is no power in nature of nothing to have made the universe, nor to reduce it to nothing, which requires equal force. But the case would be otherwise, were there a vacuum, for if it could be here, it might be there, and if here and there, why not elsewhere? Why not everywhere? So might the universe fall into nothing, by its own power: but to Him only, who had power to make it, is the glory of the power to annihilate it due. But if a vacuum can find no existence, how can air and fire descend, full against the course of their nature? Does not a positive effect, always proceed from a positively existing cause? we truly affirm then, that it is weight which carries these bodies downward, in order to unite all their particles closely, and consequently shut all the avenues to a vacuum.

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* *Literal.*

ESSAY V.

It is demonstrated that air and fire have weight, *by the greater celerity with which heavy bodies move, toward the end, than at the beginning of their motion.*

AN error, however small, committed in the beginning of any *doctrine*, increases as we proceed, and most commonly leads to very * *serious* difficulties. We experience it in regard to this subject, for philosophers, having gone astray, almost on the very threshold of natural science, ascribing levity and upward motion to the two superior elements, saw themselves afterwards much troubled to account for the natural descent of heavy bodies being quicker towards the end than at its commencement. The variety of opinions that we find in authors on this question, sufficiently demonstrates their perplexity; I, who study brevity, have no intention to bring them forward; they who like it, may read a good number of them, in the "*Natural Principles*" of Pererius†, a judicious philosopher, in which, after having quoted, he learnedly refutes them, and embraces one that he professes to acquiesce in, till he finds a better: of this I shall say something hereafter, as we go along, to shew that it is not so true as it is plausible. I now present my own, which I have with much study devised, in support of the preceding demonstrations. The quickness of motion of a heavy body increases from the beginning to the end, by the increase of elementary matter which presses on it, and by the continual multiplication of the impulse which it gives it in its descent. A figure will make my assertion clearer, (see fig. 1.) Let A A be the heavens; B B the earth; C its centre; D an iron ball, descending towards the earth; E the same descending lower; F the same again, in the middle of its descent; G the same, near the end of it; H H two lines, drawn from the centre of the earth, to the heavens, and touching the ball at D in the two extremities of its diameter; I I two similar lines, touching the ball at E; K K, two others touching it at F; and L L, two others touching it at G. It is obvious, that the ball being at D, has upon it, besides its own internal weight, the matter of the elements of air and fire, contained between the lines H H; but when at E, there is all the matter contained between the lines I I, which increases at F by the greater quantity contained between the lines in KK; and when at G the weight of the whole, contained between the

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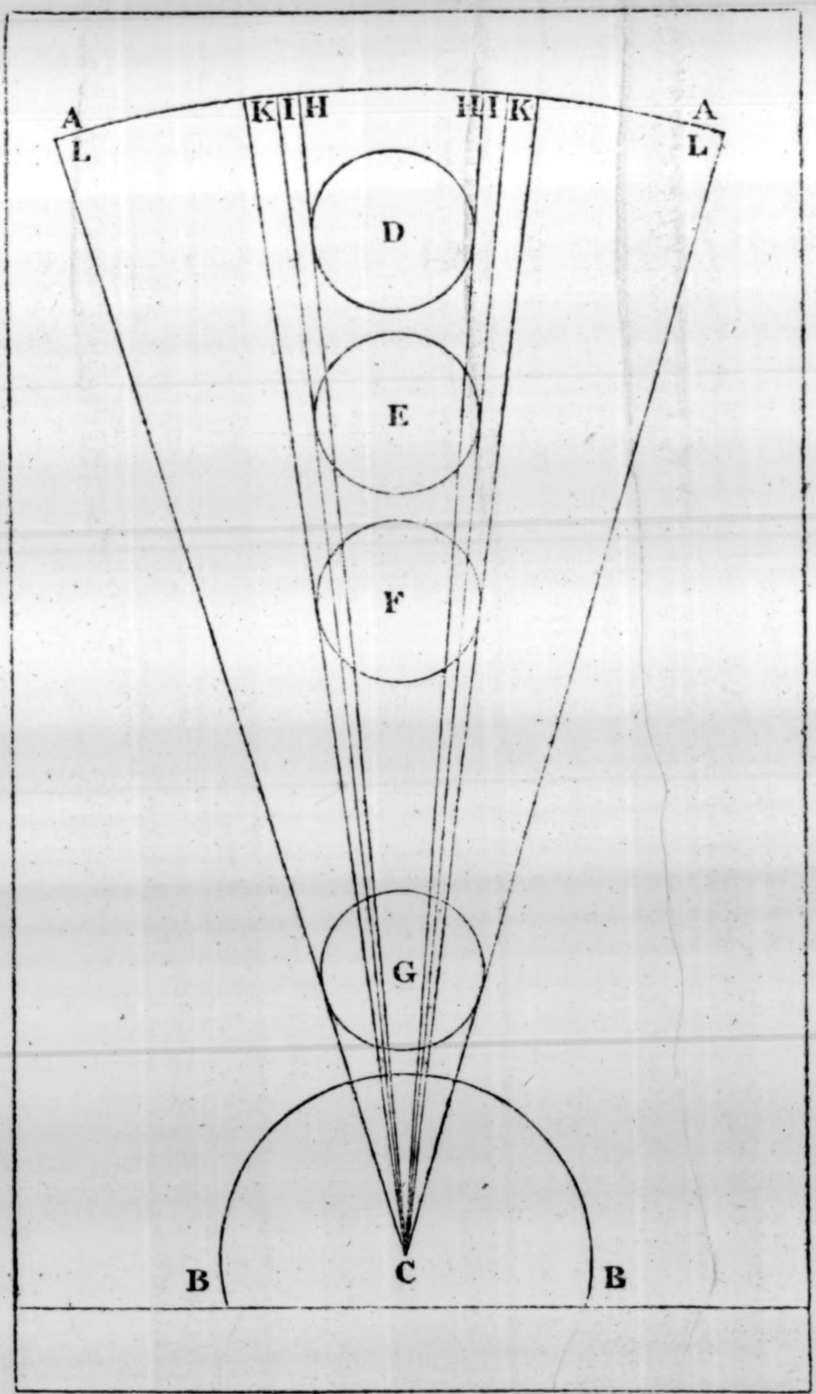
* Literally "*thorny*," "*espineuses.*"

† Benedicti Pererii de communibus omnium rerum naturalium principiis et affectionibus. *Libri* XV. in 4to. Parisiis, 1589.



lines L L presses on it, whence the quickness of its motion must increase; to which is to be added the impulse which this matter is continually giving it, as it still keeps falling down on the ball. The opinion of Pererius somewhat resembles this idea of an impulse; for he thinks that the air which follows pushes the ball; but he is mistaken in this, that air being light, and naturally tending upwards, cannot push the ball downwards, any more than a boat, towed against the stream is impelled up the river by the water, which meeting the prow, divides, and passing the sides runs continually downwards; for how can it, following this course, strike the* stern above? The other part of his assertion is no better, contending that the air, agitated by the motion, yields more readily to the thing moved. It is just the reverse, for air and water, when agitated, are capable of supporting larger weights. Ashes are suspended in water, and feathers in the air, when they are agitated, and fall down when the fluids are at rest. Surely, according to this reasoning, the motion should be slower towards the end, the agitation being then greater.

ESSAY VI.

† Gravity *is so intimately united to the first matter of the elements, that when they change from one into another, they always retain the same weight.*

My principal object, hitherto, has been to fix in the minds of all, the persuasion that air has in itself a principle of gravity, since it is from this that I purpose to derive the increased weight of tin and lead when they are calcined. But before I shew how that happens, I must extend my observation, and add, that the weight of any body is examined in two ways, by reason or by the balance. It is by reason that I have found weight in all the elements; it is reason which now induces me to deny that erroneous maxim, which has obtained from the birth of philosophy, that the elements, by mutual conversion from one into another, lose or gain weight, in proportion as they are rarefied or condensed, in the change. Armed with this reason, I boldly enter the lists, to combat the error; and I maintain that gravity is so united to the first matter of the elements, that it cannot be deprived of it. The weight that each portion receives in its cradle, it carries to its coffin. In whatever place, under whatever form, to whatever volume it be reduced, always one same weight. But not presuming that my assertions will rank with those of Pythagoras, and that it will be sufficient to have advanced them, I support them by a proof in which I think all liberal minds will acquiesce. Take a portion of earth possessing the least possible weight that can be conceived; let this earth be converted into water, by the means known and practised by nature, it is evident that this water will have weight, because all water must have it: now it will be greater or less, or equal to that in the earth. Greater, they will not say it is, (for they profess the contrary,) and I also deny it; less it cannot be, seeing that we have taken the least possible quantity: it remains therefore that it must be equal to it, which is what I intended to prove. What is demonstrated of this portion is demonstrable of two, three, or any, however great number, in short of the whole element, which is composed of nothing else, and is equally referable to the conversion of water into air, and air into fire, and *vice versâ* of these last into the others.

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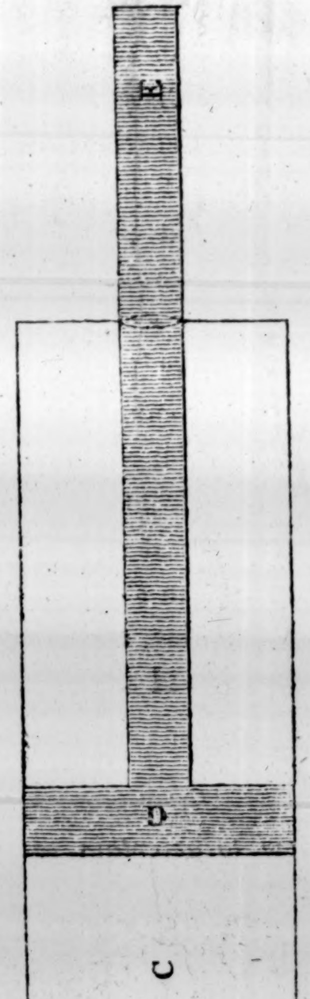
* Frapper en haut la pouppe.

† *Pesanteur,* I have translated this word by the term *gravity,* though I believe it was not used in this sense before the time of Newton; in like manner *peser,* is generally rendered *gravitate.*

ESSAY VII.

How to ascertain to what volume of air, a certain quantity of water is reduced.

Philosophers have often talked of the extension which a solid element acquires, by conversion into one more rare, and have attempted to assign its proportion: but I do not remember to have read any thing supported by *sound* reasoning or experiment. Now since in the preceding essay, I have spoken of this enlargement, the knowledge of which opens the door to many beautiful and admirable devices, I will not deprive the curious reader of a means, which I have thought of, to make the trial, and accurately ascertain to what volume a certain quantity of water can dilate itself, by transmutation into air; which experiment may serve for, and be proportionably referred to other elements. Make a brass tube of convenient size, well polished within, open at one end, and closed at the other, except a very small hole in the middle; place on the inside a piston or stopper, like that of a syringe, that may slide easily through every part, and be so correctly fitted, that no air can escape; the piston being slided to the bottom, let the tube of an æolipile, or philosophical bellows, be applied, and closely fixed to the small hole. Fill the æolipile with water, and set it on the fire; then the water becoming rarefied, and converted into air, will pass out through the little hole, and entering into the tube, in search of its liberty, push the piston by degrees, till all the water is converted into air. The capacity of the tube and æolipile, which will both be filled with it, will shew the increased bulk which this matter has acquired. Whoever wishes to know the same more easily but not so accurately, may take the intestine of a pig or other animal, and having well cleaned and flattened it, and emptied it of air, let him put it into a vessel full of water, accurately closed by a lid, having a small hole above, to let the water run out: let one end of the intestine, projecting out of the vessel by a hole on one side, be fastened to the tube of the æolipile, which, filled with water and placed on the fire, will blow into it the air, into which the water will be converted; in proportion as the intestine swells up, the water of the vessel will flow out, by the little hole in the lid, which, when col-



lected, will show the dilatation of the air in the intestine, to which the capacity of the æolipile being added, the question is solved. To these well-ascertained methods, I add the following, not unplausible one, for converting air into water, and ascertaining the diminution of volume. Let the hole of the before-mentioned tube, be closed, and the piston pushed down with great force, as far as the compression of the enclosed air will permit, and stopping it at that point, so that it cannot fly back, expose the apparatus to a frosty air, for a whole night; the air compressed on the inside, will freeze, or be converted into water, leaving only that space occupied by the air, which may remain free, (*i. e.*, unfrozen.) The measure of the water, or the ice, will give the loss of volume. I have not made this experiment: if any curious person is beforehand with me, I request he will give me an account of the result, as all the reward I ask for having taught him the method, and to the end that I may be *spared the trouble.*

A the æolipile; B its tube, entering the brass tube; C the brass tube; D its piston; E the piston rod.

ESSAY VIII.

No element gravitates in itself, and why?

I resume my argument, and say, that the examination of weights by the balance differs greatly from that by reason. The latter is only employed by the man of judgment, the *rudest clown* practises the former. This is always just, that is generally deceitful; this is attached to no circumstance of place; that commonly exercised in air, and sometimes, though difficultly, in water. It is hence, that the error which I have combated, (that air has no weight,) derives an argument capable of dazzling weak eyes, but not the clear-sighted. For weighing air in itself, and not finding it to gravitate, they have concluded that it has no weight. But let them weigh water, (which they believe to have weight,) in water itself, and they will find it equally void of it, it being most true, that no element gravitates in itself. Whatever gravitates in air, whatever gravitates in water, must contain more weight in an equal volume (in consequence of there being more matter) than the air or water, in which the weighing is performed. I proceed to deduce the cause of this, which few persons have discovered. Whatever gravitates in air (the same may be said of water) divides it, pushes it aside, and makes it give place, in order to sink to the bottom of it. This is called exercising its force and action in air. Now it is a fact, that no agent acts in its like, all action presupposing some difference. One hot body has no action in another equally hot, but rather the two will embrace and unite their actions, and by this union will no longer be two agents but only one. But if a very hot body acts in that which is less so, it is because in this case there is a dissimilitude, and in some respects an opposition; the less hot claiming the title of cold, with reference to the hotter. Thus air cannot act by its weight in air equally heavy, the two airs rather unite, and make one same weight. But whatever is heavier than air, by the dissimilitude and opposition, arising from this greater or less, will act in it, dividing, pushing it aside, and making itself a way through it to get to the bottom. But if air evince not its weight in air itself, on account of the equality of their weights, it follows *à fortiori* that it will not evince it in water, which is heavier. For even if it be placed below, it will descend no lower, the weight of the water above it only serving to compel it to seek a higher place, not allowing it a station under itself.

ESSAY IX.

Air is rendered heavy by the mixture of some matter heavier than itself.

I PURPOSE to shew that it is the air, which mixing with the calces of tin and lead, when they are calcined, increases their weight; which it would be impossible for me to do, without removing a no small difficulty that presents itself in this place. For I may be asked, how can this that I assert be, since the examination of this weight is effected by the balance, *and in air*, where air can have no weight, according to the doctrine of the preceding essay? To clear up this doubt, I say, that portions of the air may be changed and increased in weight, so that these portions, so altered and made heavier, being weighed in pure air, will afford evidence of their weight. But what is this change which causes it to become heavier? I remark, that it may happen in three different ways; either by the mixture of some heavier foreign matter; by the compression of its parts; or by the separation of its lighter portions. Let us speak, first, of the first, and then of the two others. It is certain that the air is capable of containing many matters heavier than itself; such are the vapours and exhalations which rise from the water, or the earth. A portion of air imbibed with these matters, will weigh more than an equal portion of another air containing nothing of the kind; like as sea-water is heavier than the water of fresh rivers; the former containing much salt, which the latter is free from. Observe, I pray you, how, in cloudy weather, at first opening your high windows, the air enters your chamber loaded with fog! Do you not conclude that this weighs more than the other, since it cleaves it and falls down in it? Fill a balloon with this cloudy air, it will weigh more than the same filled with pure unmixed air. Reason accords with this experiment, saying thus; if to two equal weights, we add two unequal weights, the two weights will be unequal, and that will be the greatest, to which the greatest weight has been added. If we take, for instance, two portions of the same air, each equal to ten cubic inches, and add to one of them two inches of water, and to the other two inches of air, who but perceives that these two portions will be very equal in volume, but unequal in weight, and that the one containing the water will be the heaviest? This is so manifest, that I abstain from saying any thing more about it, especially as this mode of increasing weight, has not much to do with our subject. Proceed we, therefore, to the others.

ESSAY X.

Air is rendered heavy by the compression of its parts.

The second mode by which air increases in weight, is by the compression of its parts; for nature has willed, for reasons known to herself, that the elements should dilate and contract, within certain limits, which she has prescribed to them. Within this space, we see a portion of an element now narrowly contracted, now widely distended. Observe the pot, half full of water, under which the cook is about to make a large fire. The water will dilate till it runs over the brim: but if the fire be extinguished, it will contract and return to its original bulk. Take this syringe in which the piston is pushed half way down, and the opening in front well closed; push it forcibly, you will reduce the enclosed air to a small compass. Draw back the piston towards you, and though you do not pull it out, yet you will cause the air to dilate to more ample dimensions than it had before. The air being thus compressed, do you doubt that it will have sensible weight in a free air, since it contains more matter in an equal space? If the reasons already given in the eighth essay be not sufficient for you, make the experiment. Fill a balloon with air, strongly compressed by means of a pair of bellows, you will find it weighs heavier when full, than empty. And by how much? By so much as the additional quantity of air in the balloon, weighs, in proportion more than that which is free, under an equal volume. Many have indeed remarked the greater weight of the balloon when full than when empty; but it has not come within my observation, that any one, hitherto, has known the cause of it. Leaving aside persons of low account, Dr. Scaliger who possesses the true genius of Aristotle, did not understand it; for in the hundred and twenty-first exercitation against Cardan, he follows the beaten track, holding that pure air is light, and that the balloon gains weight because the air which is next the surface of the earth, such as is forced into the balloon by the bellows, is mixed with vapours, and those little terrestrial bodies clearly discernible in the sun's rays*. But alas! what good does this mixture do him? since the experiment is made in an air perfectly similar: certainly it could evince no weight in it unless compressed. If the balloon were forcibly filled with the purest air in nature, or even with elementary fire, reason says it still must have weight, if balanced, in the first instance, in the same, and in the second, in fire itself. This compression of air, is a fertile field in which ingenious minds will collect rare devices. From hence the Sieur Marin, Citizen of Lisieux, has derived his Arquebuss, which I discovered many years since, before the Sieur Flurance had described it*, but which far excels that of Marin, (I say it without vanity,) in having much greater force. I could acquaint the reader with another elegant and profitable invention, which I have derived from the same source, but I am purposely silent concerning it; hoping one day to have the happiness of presenting a most humble petition to his Majesty, that he will honour me with the privilege of the exclusive use of it, for a certain time, in order somewhat to reimburse me for the expense I must be at in bringing the said invention, as well as some others which till that time I keep secret, into use.

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* "Purum aërem levem esse. Inflatū utrem plenū esse aëris impuri: "sive ab homine sufflatus sit: udi enim multū vehit secū: sive à folle. "Satis enim patet, aërem hūc, qui circū terræ est superficiem, vaporibus "atque terrestribus corpusculis mistum esse: quæ in solis radiis apparent "manifestò." *Ivl: Caes: Scalig: de Svbtil: ad Hier: Cardan, Exercit:* CXXI. p. 181. Lutetiæ, M. D. LVII.

[*To be continued.*]

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* This relates to an air-gun invented by Jean Rey. He quotes the work of David Rivault, Sieur de Flurance, a native of Laval on the Maine, but descended from an ancient family in Britanny, a counsellor of state, and preceptor to Louis XIII. The work is called *Elements of Artillery, &c.* 8vo. Paris, Adrien Denis, 1608. It is dedicated to the Duc de Sully, and the preface contains the history of the invention and first use of fire-arms, ancient and modern. Flurance left the service of Louis XIII, in consequence of a blow he received from the king, for having kicked a favourite dog, which was troublesome to him, whilst giving the prince a lesson. He was afterwards recalled to court, and died at Tours, in January 1616, at the age of 45.

The Editor of the reprint of Rey's essays, adds, that air-guns were discovered in France, by the Sieur Marin Bourgeois, an inhabitant of Lisieux in Normandy, whom Flurance calls "a man of most rare judgment "in all sorts of inventions, of the most artful imagination, and of consummate dexterity in handling the tools of every art known in Europe, "without having learnt of any master. He is an excellent painter, statuary, musician and astronomer, and works more delicately in iron and "copper than any other artist that I know. The king, Louis XIII, has "a table of polished steel, made by him, in which his majesty is represented to the life, without the help of engraving, modelling, or painting, "but merely by fire, which this admirable workman applied more or less "to the different parts, as the figure required to be bright, brown, or "obscure. He has a sphere made also by him, in which the motions of "the sun, moon and stars are represented. He has, likewise, invented "a musical scale, for his own use, by means of which he writes down in "a manner only known to himself, the airs of all songs, and plays them "afterwards on his viol, in concert with those who play the other parts, "without their knowing any thing of his method, or he understanding "their science." Flurance saw Marin Bourgeois' air-gun at Lisieux in 1607, and having become intimate with him, obtained the description of it, which he published in 1608. Experiments were made with the air-gun in the presence of the king and one of his ministers. "It is right," adds the editor, "to publish this anecdote, so honourable to the artist, and which secures to us the priority of the invention."

ART. V. *Contributions towards the Chemical Knowledge of Mineral Substances. By the late* MARTIN HENRY KLAPROTH. Vols. IV. V. *and* VI.

[It has, we believe, been frequently regretted that no English translation of the three last volumes of *Klaproth's Analytical Essays* has hitherto appeared; in compliance, therefore, with the suggestion of several of our chemical readers, we propose to lay before them an account of the principal analyses contained in those volumes; giving sometimes an entire translation of the original, at others an abstract, and sometimes merely the results of the author's experiments; being in these respects guided by the novelty and importance of the details, and by the originality and efficacy of the manipulations. We trust that the chemical student will especially derive advantage from an acquaintance with the latest labours of this celebrated and accurate analyst.]

1. *Chemical Examination of Electrum, a native Alloy of Gold and Silver.*

THE term *electrum*, commonly applied to amber, has also been used to denote an alloy of gold and silver. "*Omni auro inest argentum vario pondere: ubicunque quinta argenti portio est* ELECTRUM *vocatur* *," says Pliny; whence it would appear that the term is only properly applicable to the alloys containing excess of gold, which is the case with the *electrum* of Schlangenberg in Siberia, where it occurs native, of a pale gold colour, in small plates, imperfect cubes, *&c.*, associated with a grey coarse-grained sulphate of baryta, and also with a splintery variety of grey horn-stone: the matrix of the specimen employed in the following analysis was sulphate of baryta. To separate any free silver or gold it was first digested in nitric acid, to which muriatic acid was afterwards added; it was then fused with borax.

A piece of the electrum thus purified and weighing 25 grains, was beaten flat and boiled in nitric acid, which exerted no action upon it; an equal portion of muriatic acid was then added, but still without effect.

b. The electrum was then fused with the addition of its weight of silver, laminated, and boiled with nitric acid, which only dissolved the added silver, and left the original twenty-five grains of electrum untouched.

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* Lib. xxxiii. cap. iv. sect. xxiii.

c. It was then melted with thrice its weight of silver, laminated, and digested in nitric acid, which now effected, a complete separation, leaving the gold in heavy brown scales, which being washed, and fused into a button, weighed 16 grains.

d. The *silver* was separated from the nitric solution by the immersion of a plate of copper, and amounted to 84 grains: deducting the 75 grains, which were added, 9 grains remain as the proportion of that metal contained in the native alloy; hence the electrum consists of gold 64 grains

silver 36 . .

100

As this alloy resists the action of nitric, and even of nitro-muriatic acid, it is evidently not a mere mixture of gold and silver, but a peculiar definite ore in which those metals are chemically combined.

2. *Chemical Analysis of the Pacos, or red Silver Ore of Peru.*

I am indebted to M. von Humboldt for the specimen of the Peruvian silver ore, which is called *pacos*, employed in these experiments. When minutely examined, it has the appearance of a brown oxide of iron traversed by filaments of native silver, which is therefore easily separable by amalgamation.

a. 100 grains of this ore heated to redness lost 8.5 grains, and acquired a brown red colour.

b. 200 grains digested in nitric acid furnished a colourless solution, when filtered, from which muriate of soda threw down 37.25 grains of muriate of silver, equivalent to 28 grains of metallic *silver*. The remaining liquid supersaturated by carbonate of potassa, afforded a light brown precipitate of *oxide of iron*, which after ignition weighed three grains.

c. The portion which had resisted the action of nitric acid was digested in boiling muriatic acid; nine grains remained undissolved, consisting of seven grains of impalpable *siliceous* earth and two grains of coarse sand, amongst which minute octoëdral crystals were perceptible, consisting probably of ferriferous titanium, and some small particles of sulphur, which burned away with a blue flame. The muriatic solution when cold, was duly diluted and precipitated by carbonate of soda; the precipitate, collected, dried, and ignited, gave 139 grains of *oxide of iron*. The remaining fluid boiled with excess of carbonate of soda afforded a trace of *oxide of manganese*: 100 parts of this ore therefore contain,

Silver	14.
Brown oxide of iron . . .	71.
Silica	3.50
Sand, &c.	1.
Water	8.50
	98.

Chemical Analysis of the Hepatic Mercurial Ore from Idria.

The specimen employed in the following analysis was compact, and its colour intermediate between cochineal red and lead grey; it is opaque, gives a dark red streak, and acquires a shining surface when rubbed; it is soft, tasteless, and of a specific gravity=7.100 It is susceptible of a bad polish, and then appears of a liver brown colour.

A. 1000 grains of this ore, distilled with half its weight of iron filings, gave 818 grains of pure *mercury*. The residuary sulphuret of iron was mixed with a black powder.

B. *a.* 100 grains of the ore in fine powder were boiled with 500 grains of muriatic acid, which occasioned the evolution of sulphuretted hydrogen: 100 grains of nitric acid were then gradually added, by which the whole of the ore was dissolved with the exception of a black residue of 10 grains; this residue was carefully heated upon a porcelain capsule, so as to burn away the sulphur only: there remained three grains of carbonaceous matter, which, when more strongly ignited, left one grain of red ash.

b. The above nitromuriatic solution was precipitated by muriate of baryta, and the sulphate of baryta thus produced weighed after ignition 46.5 grains, whence it appears that 6.5 grains of sulphur were acidified by the nitric acid: if to these we add the above seven grains which were burned, and estimate that lost in the sulphuretted hydrogen as equal to 0.25 grains, the proportion of *sulphur* in the ore may be considered as = 13.75 grains, *per cent.*

C. *a.* 1000 grains of the ore were gradually heated to redness in a retort connected with the pneumatic apparatus: after the expulsion of the atmospheric air, 34 cubic inches of sulphuretted hydrogen were evolved, besides a portion absorbed by the water.

b. The receiver contained some globules of mercury: the neck of the retort contained a mixture of moist black sulphuret of mercury, globules of metallic mercury, and some particles of cinnabar; the mercury, mechanically separable, weighed 317 grains. In the upper part of the neck of the retort was a compact sublimate of pure cinnabar, weighing 236 grains.

c. There remained in the retort a light black powder, weighing 39 grains, which, after incineration upon a capsule, left 16 grains of a brownish grey, earthy powder; hence the portion of burned carbonaceous matter may be considered as = 23 grains.

d. This earthy residue was digested in muriatic acid; there remained *silica* weighing, after having been ignited, 6.5 grains.

e. The muriatic solution, which was of a greenish colour, was supersaturated by ammonia, which produced a brown precipitate, leaving the supernatant liquid of a pale blue colour. The precipitate was digested in hot solution of potash, and there remained *oxide of iron*, which having been brought to the magnetic state, weighed two grains.

f. Muriate of ammonia being added to the alcaline liquor, *alumina* was precipitated, weighing, after having been ignited, 5.5 grains.

g. The ammoniacal liquor, after supersaturation with muriatic acid, afforded upon a piece of immersed zinc, 0.20 grains of metallic *copper*.

From the above experiments it appears, that 1000 parts of the compact hepatic ore of mercury from Idria contain

Mercury . .	A. . . .	818.
Sulphur . .	B. *b* . . .	137. 50

Carbon	C. *c*	23.
Silica	*d*	6.50
Alumina	*f*	5.50
Oxide of iron	*e*	2.
Copper	*g*	0.20
Water and loss		7.30
		1000.00

To the above analysis Klaproth adds some remarks respecting the state of the mercury in the red sulphuret.

Chemical Examination of the Lamellar Red Copper Ore of Siberia.

a. 100 grains of selected crystals of the above ore were digested in cold muriatic acid: the mixture became warm without effervescing; the ore lost its red colour, becoming greyish white, and the acid acquired a dark brown colour. A fresh portion of acid was added, by which the grey portion of ore was taken up, and some particles of metallic copper remained undissolved, which however ultimately disappeared on continuing the digestion.

b. On pouring the solution into water, it became decomposed, and deposited a white precipitate of sub-protomuriate of copper, which, upon adding excess of potassa, acquired an orange colour; the whole was then poured upon a filter, and the residue being washed and dried, proved to be protoxide of copper, of a rhubarb-yellow colour.

B. From these experiments it appears that the copper in the above ore is in the state of protoxide. To ascertain the relative quantity of oxygen to that of metal, 100 grains of the ore were carefully digested in a stopped phial with muriatic acid, so as to leave the metallic copper undissolved, which was collected in small crystallized particles, and weighed 12 grains. The solution, containing 78 grains of pure red copper ore, was heated to its boiling point, and nitric acid added drop by drop, till it became of a grass-green colour; it was then diluted, and after a small addition of muriatic acid, was heated and decomposed by zinc, by which the copper was separated, and after being washed and quickly dried, was found to weigh 71 grains

As no other ingredient was to be detected, it appears that the 78 grains of ore contained seven of oxygen.

If, therefore, we exclude the metallic copper, which is to be regarded as mixed and not chemically combined with the ore, the composition of 100 parts of the octoëdral red copper ore of Siberia is as follows:

Copper	91
Oxygen	9
	100

Analysis of the fibrous Blue Copper Ore of Siberia.

This ore occurs in the Turjin mines of the Ural mountains, generally in globular concretions of dark blue crystals, which appear to be somewhat oblique four and six-sided prisms. The specimen selected for examination was coarsely powdered, and washed to separate some adhering earthy particles.

A. 100 grains ignited in a covered crucible, acquired a black lustre, and lost 30 grains.

B. Nitric acid dissolved the ore with effervescence, forming a transparent blue solution, which was not affected by the addition of acetate of baryta, acetate of lead, nor nitrate of silver.

C. 100 grains of the ore dissolved completely in muriatic acid, forming a dark green solution, which became bluish when diluted. On supersaturation with caustic ammonia, the precipitate first formed was entirely redissolved; the ammoniacal solution was then supersaturated by sulphuric acid, and a plate of iron immersed, which caused the separation of 56 grains of *copper*.

D. 100 grains of the ore were put into a counterpoised phial, containing a sufficient quantity of sulphuric acid diluted with four parts of water, and were gradually dissolved without the aid of heat. The evolution of *carbonic acid* produced a loss of weight equal to 24 grains: the solution precipitated by zinc gave 56 grains of *copper*.

E. 100 grains of the ore heated in a small glass retort connected with the pneumatic apparatus, afforded about 35 cubical inches of carbonic acid gas: the intermediate recipient contained several drops of pure water. The ignited residue was black and shining, and weighed 70 grains. As this residue has already been shown to consist of pure oxide of copper, of which the oxygen constitutes a fifth part, the above 70 grains are obviously composed of 56 grains of copper and 14 of oxygen. Hence the following are the constituents of the ore:

Copper	56
Oxygen	14
Carbonic acid	24
Water	6
	100

Contrasting these results with those afforded by the analysis of malachite*, it appears that the blue carbonate of copper differs from malachite in containing more carbonic acid and less water.

Analysis of a Green Copper Ore from Siberia.

This ore occurs in the Turjin mines; it is massive, has little lustre, is translucent in thin fragments, brittle, and of a verdigris green colour inclining to sky-blue.

A. *a.* 100 parts heated to redness, lose 24 parts, and acquire a black colour.

b. It is slowly acted upon by nitric acid, evolving air-bubbles and leaving a residue of pure silica. The weight of the *carbonic acid* is 7 *per cent.*, which, deducted from the 24 *per cent.* lost during ignition, gives 17 *per cent.* as the weight of the *water.*

B. *a.* 100 grains digested in nitric acid leave 26 grains of *silica.*

b. The nitric solution, of a pure blue colour, was supersaturated by ammonia; the precipitate at first thrown down was perfectly redissolved, and a deep blue solution was obtained.

c. This solution, supersaturated with sulphuric acid and precipitated by zinc, afforded 40 grains of *copper*, which are equivalent to 50 grains of peroxide of copper. The following, therefore, are the components of this ore:

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* *Beiträge*, 2 B. s. 290.

Copper	40
Oxygen	10
Carbonic acid	7
Silica	26
Water	17
	100

The silica in the above mineral is not merely mechanically blended, but forms one of its chemical components.

Analysis of the Copper Glance of Rothenburg.

The compact variety of the above ore, of a greyish black colour, soft, giving a dull black streak, and of a specific gravity of 4.685, was employed in the following experiments; it also occurs at Rothenburg in hexaëdral prisms.

A. *a*. Having ascertained the absence of silver, lead, antimony, *&c.*; and that copper and sulphur are the components of this ore, 100 grains were digested in muriatic acid, the action of which was aided by the occasional addition of some drops of nitric acid, till the ore appeared decomposed. The liquid was then poured off, and the remaining *sulphur* having been digested in a fresh portion of muriatic acid, was washed and dried: it weighed 22 grains, and burned away without any appretiable residuum.

b. The colour of the solution, at first brown, became green and blue by dilution; the addition of ammonia occasioned a precipitate perfectly redissoluble in excess of the alcali, with the exception of some brown flocculi of oxide of iron, which were found equal to one-half grain of metallic *iron*. The ammoniacal liquor supersaturated by sulphuric acid, and decomposed whilst warm by the insertion of a plate of iron, gave 76½ grains of metallic *copper*.

B. 100 grains of the same ore were reduced to powder, and distilled to dryness with six parts of dilute sulphuric acid (composed of two parts of concentrated acid and one of water). The residue was digested in water, and a plate of zinc immersed in the filtered solution, caused the precipitation of the same quantity of copper as in the previous process. Hence 100 parts of this ore contain

Copper	76.50
Iron	0.50
Sulphur	22.00
Loss	1.00
	100

ART. VI. *Description of the Balance represented in* Plate V. fig. 1.

A GOOD pair of scales is an essential implement to the chemist and mineralogist, but it has hitherto been scarcely attainable, except at a price far too considerable for the generality of purchasers. We are indebted to our friend Mr. Children for the drawing of the beam represented in the above-mentioned plate, and which will, we believe, be found to possess considerable accuracy in a small and convenient compass, and at a very moderate expense.

The beam, which is of platinum and made so light as to be extremely sensible, is at the same time rendered sufficiently strong by its form. The adjustments for bringing the points of suspension of the scale pans, equidistant from, and in a right line with the axis, are performed by the screws *a* and *b* at the ends of the beam; these screws also serve to strengthen the curved ends of the beam and prevent their bending. The axis of the beam is a piece of very hard steel, of an equilateral triangular form, passing through the beam and bearing on agate planes; the knife edges are ground to an angle of 120°, which has been chosen thus obtuse from the liability of a more acute edge to be broken when suddenly lowered upon the agates. The ends of the axis are chamfered from the top to the knife edge, so that when the beam is lowered upon its bearings, they are clear of the lifting frame. An index or pointer descends from

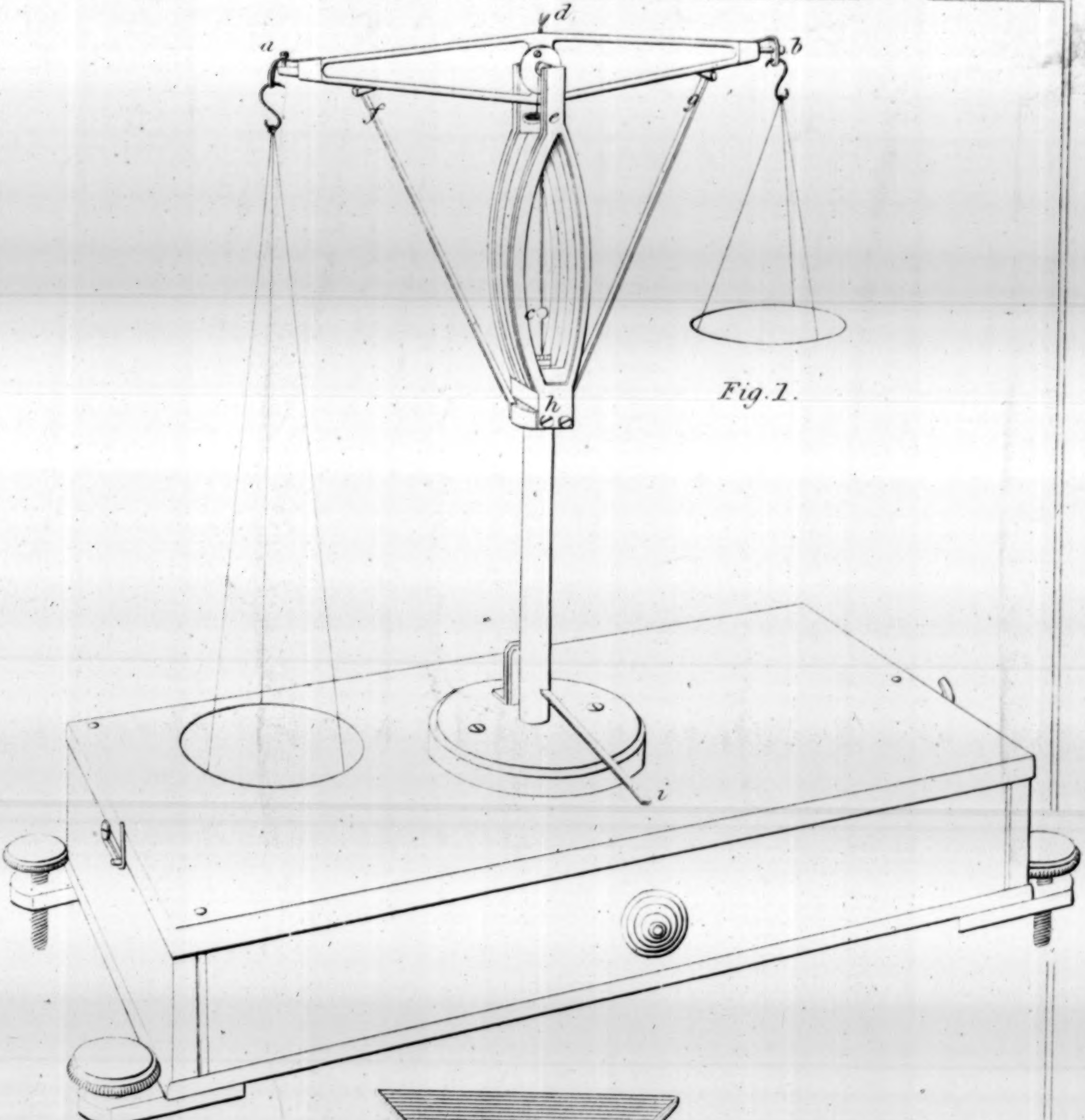

Fig. 1.

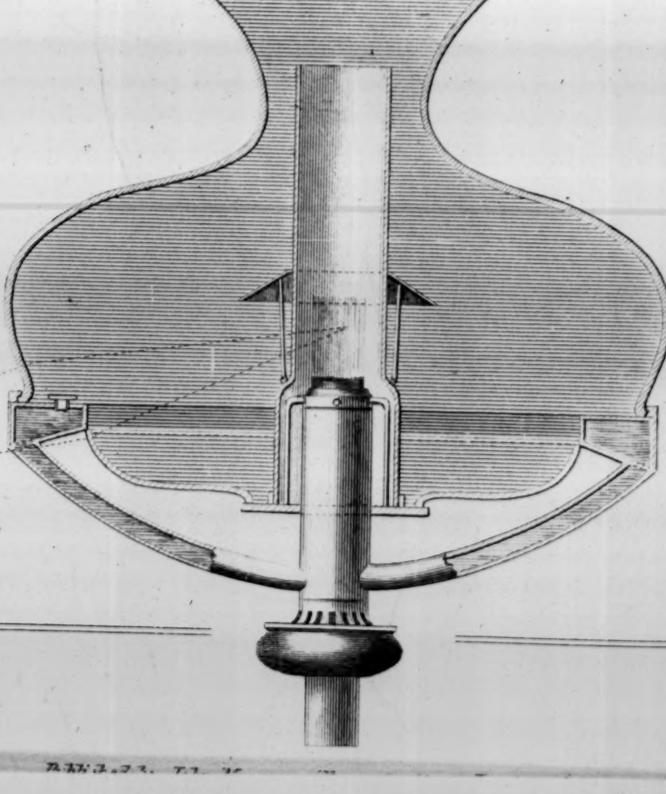

Fig. 2.

Jˢ Basire sc.

the beam to a divided scale; upon this index is screwed the ball *c*, the office of which is to adjust the oscillation of the beam, or in other words to give it its required sensibility; should the index not exactly coincide with the middle division of the scale, it may be adjusted by turning the piece of wire *d*, at the top of the beam. The lifting frame, *e f g h*, is pressed upwards by an internal spring, its use being to lift the beam from the agates, when not in use, or when weights are put into the scale pans: this frame is lowered by the lever *i*, and is retained in that position by turning the lever into the notch on one side. At the bottom of the short stringed pan is a hook for the convenience of attaching substances whilst ascertaining their specific gravities. The whole instrument is covered with a glass case, and provided with a level for ascertaining the horizontality of the agates, and with tweezers, and a box of platinum weights from $\frac{1}{100}$ of a grain up to 100 grains. The price of this apparatus as manufactured by Mr. Robinson, of Devonshire-street, Portland-place, is £6, with the beam and weights accurately adjusted, or £4 when unadjusted.

ART. VII. *On the Magnetism impressed on Metals by Electricity in Motion; read at the public Sitting of the Academy of Sciences, 2d April,* 1821. *By* M. BIOT.

WHEN the electric current, evolved from a voltaic battery, is transmitted through any metallic bodies whatsoever, it gives them instantaneously a magnetic virtue; they become then capable of attracting soft and unmagnetized iron. This curious fact was discovered by M. Oersted. If we expose to these metallic bodies, a magnetic needle, they attract one of its poles, and repel the other, but only relative to the parts of their surface to which the needle is presented. Needles of silver or copper are not affected, but merely those susceptible of being magnetized. These effects subsist only under the influence of the electrical current. If we suspend the circulation of the electricity, by breaking off the communications established between the opposite poles of the voltaic apparatus, or even if we retard considerably its velocity, by joining its poles with bad conductors, the magnetic power instantly ceases, and the bodies which had received it return to their usual state of indifference.

This simple sketch already displays many new properties. All the processes hitherto employed to magnetize bodies had produced an effect on only three pure metals, iron, nickel, and cobalt, and on some of their compounds, steel for example, which is merely a carburet of iron. Till now it was never possible to render silver, copper, or the rest of the metals magnetic. But the electric current gives all of them this property; it bestows it transiently by its presence; and, as we shall presently see, it diffuses it through the whole mass, in a manner equally singular, and which has no resemblance to what is produced, when we develope magnetism by our ordinary processes, which consist in longitudinal friction with magnetic bars.

To produce these novel phenomena in the simplest manner, we must with M. Oersted, establish a communication between the two extremities of the voltaic apparatus, by a simple metallic wire, which may be easily directed and bent in all directions. We place afterwards in the neighbourhood of the battery, a very sensible magnetic needle, horizontally suspended. As soon as this is settled in the direction due to the magnetic force of the terrestrial globe, we take a flexible portion of the conducting, or *conjunctive* wire, as M. Oersted calls it, and having stretched it parallel to the needle, we bring it gently near it, either from above, from below, from the right, or from the left. We shall see an immediate deviation of the needle; but what is not the least remarkable circumstance, the direction of this deviation differs according to the side by which the conjunctive wire approaches it. Duly to comprehend this astonishing phenomenon and to fix its peculiarities with precision, let us suppose that the conjunctive wire is extended horizontally from north to south, in the very direction of the magnetic direction in which the needle reposed, and let the north extremity be attached to the copper pole of the trough, the other being fixed to the zinc pole. Imagine also that the person who makes the experiment looks northward, and consequently towards the copper or negative pole. In this position of things, when the wire is placed above the needle, the north pole of the magnet moves towards the west; when the wire is placed underneath, the north pole moves towards the east; and if we carry the wire to the right or the left, the needle has no longer any lateral deviation, but it loses its horizontality. If the wire be placed to the right hand, the north pole rises; to the left, its north pole dips; and in thus transporting the conjunctive wire all around the needle, in directions parallel to one another, we merely present it to the needle, by the different sides of its circular contour, without affecting in the least the proper tendency of the needle towards the terrestrial magnetic poles. Since then the deviations observed in these successive positions are first of all directed from right to left, when the wire is above the needle; then from above downwards, when the wire is to the left; from the left to the right, when the wire is beneath; and, finally, from below upwards, when it is to the right hand, we must necessarily conclude from these effects, that the wire deranges the needle, by a force emanating from itself, a force directed transversely to the length of its axis, and always parallel to the portion of its circular contour, to which the needle is opposite. M. Oersted drew this inference from his first observations.

Now this revolutive character of the force, and revolutive according to a determinate direction, in a medium which like silver, copper, or other pure metal, seems perfectly identical in all its parts, is a phenomenon very remarkable, of which we had heretofore only one singular example in the deviations which certain bodies impress on the planes of polarization of the luminous rays. The first fact of the magnetism transiently impressed upon the conjunctive wire by the voltaic current, might have offered itself to a vulgar observer. I do not know whether some traces of this property may not have been previously perceived and indicated; but to have recognised this peculiar character of the force, and to have delineated it, agreeably to its phenomena, without hesitation and uncertainty, is the praise which truly belongs to M. Oersted, and which constitutes a condition entirely new in the movement of electricity.

As soon as this beautiful discovery was known in France, England, and Germany, it excited the most lively sensation among men of science. One of our colleagues, in particular, M. Ampere, ardently verified it in all its circumstances. Seizing with sagacity the revolutive character of the force impressed on the conjunctive wire, he directed it with judgment, and skilfully developed the consequences which flowed from this property. His researches, which preceded those of the other French philosophers, have considerably occupied the Academy; but as the order of exposition, occasioned by the mutual dependence of the phenomena, hinders me from beginning with them, I have endeavoured to compensate for this inversion by rendering justice at once, to labours which have anticipated and facilitated others.

In the above experiments which M. Oersted had made, the conjunctive wire is presented to steel needles, previously magnetized. It may be asked, if the action then exercised is proper to the conjunctive wire, as the action of a bar of steel tempered and magnetized is proper to this bar, or if the action is communicated to the wire by the presence of the magnetic needle, as we see soft iron, which exercises no magnetic power of itself, acquire transiently this power in the presence of magnets? To decide this question it was necessary to examine whether a body, not magnetic in itself, but capable of becoming so by influence, soft iron for example, would experience a sensible action at the approach of a conjunctive wire, traversed by the voltaic current. This was effected by M. Arago, who shewed that filings of iron are attracted by these wires; a simple but important fact, which defines clearly one of the characters of the force by which the phenomenon is produced.

I shall now proceed to indicate briefly what has been done towards completing the analysis of the electro-magnetic forces.

The first thing which we must determine, is the law according to which the force emanating from the conjunctive wire decreases at different distances from its axis. This inquiry has been the object of experiments which I made along with M. Savart, already known to the Academy by his ingenious discoveries in Acoustics. We took a small needle of magnetized steel in the form of a parallelogram, and to ensure its perfect mobility, we suspended it under a glass bell, by a single fibre of the silk-worm, and gave it at the same time a horizontal direction. Then in order that it might be entirely at liberty to obey the force emanating from the conjunctive wire, we screened it from the action of the magnetism of the earth, by placing a magnetized bar at such a distance, and in such a direction, that it exactly balanced this action. Our needle was thereby placed in the same freedom of movement as if there did not exist any terrestrial globe, or as if we had been able to transport ourselves with it to a great distance in space. We now presented it to a conjunctive cylindrical wire of copper, stretched in a vertical direction, and to which we had given such a length, that its extremities necessarily bent in order to attach them to the poles of the electric apparatus, should have, in consequence of their distance, so feeble an action on the needle that it might be neglected with impunity. This disposition represented therefore the effect of an indefinite vertical wire, acting on a horizontal and independent magnetic needle. As soon as the communication of the voltaic current was completed, the needle turned transversely to the axis of the wire, conformably to the revolutive character indicated by M. Oersted; and it set itself to oscillate around this direction, as a clock pendulum, moved from the perpendicular, oscillates round the vertical by the effect of gravitation. We counted with an excellent seconds watch of M. Breguet, the time in which a certain number of these oscillations, twenty for example, were performed; and by repeating this observation, when the wire and needle were at different distances, we inferred the decreasing intensity of the force, precisely as we determined by the oscillations of the same pendulum, the variations of gravity at different latitudes. We thus found that the force exercised by the wire was transverse to its length, and revolutive, as M. Oersted had observed; but we discovered besides that it decreased in a ratio exactly proportional to the distance. However, the force which we thus observed was in reality a compound result; for on dividing in imagination the whole length of the conjunctive wire, into an infinity of segments of a very small altitude, we perceive that each segment ought to act on the needle, with a different energy according to its distance and direction. Now these elementary forces are precisely the simple result which it is important to know, for the total force exercised by the whole wire, is merely the sum of their actions. Calculation enables us to remount from this resultant to the simple action. This has been done by M. Laplace. He has deduced from our observations, that the individual law of the elementary forces, exercised by each section of the conjunctive wire, was the inverse ratio of the square of the distance; that is, precisely the same as we know to exist in ordinary magnetic actions. This analysis shewed that in order to complete the knowledge of the force, it remained to determine if the action of each section of the wire was the same, at an equal distance, in all directions; or if it was more energetic in a certain direction than in others. I have assured myself by delicate experiments that the last is the case.

What we now know of the law of the forces, is sufficient for explaining and connecting together a multitude of results, of which I now proceed to indicate briefly some of the most curious. For example, let us conceive as we have done above, an indefinite conjunctive wire, stretched horizontally from south to north. Let us present laterally to it a magnetic needle, of a cylindrical shape, and suspended so that it can take no movement but in the horizontal direction. For greater simplicity, let us withdraw it from the influence of the terrestrial magnetism, by neutralizing this influence with the action of a magnet suitably placed. This being done, when the needle rests at the same height as the wire, so as to point exactly to its axis, it is neither attracted nor repelled; but if we raise it above the wire, it presents one of its poles to it, and makes an effort of approximation. If, on the contrary, we sink it below, the needle turns about, to present its other pole, and is then attracted anew. But if we constrain it to present the same pole as at first, the needle is repelled, and the effects are precisely inverse on the right and on the left hand of the wire.

If instead of transmitting the electrical current across a simple wire, we make it pass through tubes, plates, or other bodies of a sensible breadth, whose surfaces are composed of parallel right lines, we find that all these bodies act on the magnetic needle, as bundles of wires parallel to their length would do; which proves that the power developed in them by the electrical current is exerted freely through their very substance, and is not weakened by its interposition, as the radiation of heat through hot bodies is enfeebled and intercepted by the interposition of these very bodies.

Instead of leaving the needle in the preceding experiment at liberty to move, fix it invariably, but render the conjunctive wire mobile, by suspending it on two points; then it will be the latter which will move towards the needle, or recede from it. In fact, it is a general law of mechanics, that reaction is always equal to action. If the wire attract or repel the needle in certain circumstances, the needle ought in the same circumstances to attract or repel the wire. This experiment belongs to M. Ampere.

Now, let us operate no longer with a magnetic needle on the wire placed in its position of mobility, but let us expose it to the magnetic action of the terrestrial globe, which is known to be perfectly similar to that of a common magnet whose poles are very distant. This force will likewise make the conjunctive wire move according to the same laws, at least if it be sufficiently freely suspended, and it will impress on it a determinate direction relative to the plane of the meridian, just as it would direct any other magnetic body. This result was realized by M. Ampere*.

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* The accuracy of this result has been questioned by some able philosophers in this country, on both theoretical and experimental grounds.

Finally, instead of presenting a conjunctive wire to a magnet, present two conjunctive wires to one another, in parallel positions. Then if the revolutive direction of the force be the same for the two wires, they will both concur in giving one direction to a magnetic needle placed between them; but if the direction of the revolutive movement be opposite in each, they will tend to turn the needle in opposite directions. These are simple consequences of the law of the forces. Now on trying these two arrangements, M. Ampere has found that in the first the two wires come together, and that in the second, they mutually repel each other. Thence we must make two inferences; first, that the wires exert on each other actions perfectly analogous to those which they exercise on magnetic needles; and next that the distribution of these forces in each of their particles is analogous as to direction, with what it is in magnetic needles themselves. These two new conditions relative to the nature of the force, render this experiment very important.

In the different arrangements which we have just described, the conjunctive wires and the magnets attract or repel principally by their most contiguous parts; for with regard to the rest, their distance rapidly diminishes their action. Hence it is evident that we should augment the energy of the effects, if we approximated together the different parts of the conjunctive wire, preserving to them however the same general line of action. M. Ampere has also verified this position, by coiling the conjunctive wire in the form of a flattened spiral, on the plane of whose contours he acts with magnets, as on the side of a single wire.

M. Ampere's mode of operating, consists in bending a portion of wire, about two or three feet long, into a circular form, recurving its extremities so as to make each point dip into a little cup of mercury, which serves as pivots on which the circle is suspended and round which it may revolve. Into one cup, the ordinary wire from the copper end, and into the other, the wire from the zinc end of the voltaic trough is plunged. The plane of the circle then slowly places itself, according to M. Ampere, at right angles to the magnetic meridian.—TRANSLATOR.

Among the arrangements which he has thus formed, one of the most remarkable consists in winding the conjunctive wire around a cylinder of wood or glass, forming an elongated spiral. Then the force emanating from each point of the thread being always directed transversely to its length, becomes in each element of the spiral, perpendicular to the plane of the coils, and consequently parallel to the length of the spiral itself. Farther, on account of the revolutive character of the force, all the inner points of the different rings exercise, in the interior of the spiral, forces which are equal, and operate in the same direction; whilst in their action exteriorly, the forces emanating from the different points of each ring, oppose and weaken each other greatly by their obliquity. Thus the result of all these actions ought to be much more energetic within the spiral than outside of it; a consequence which actually happens. If we place in the interior of a spiral, unmagnetic steel needles, they will acquire in a few instants a permanent and very perceptible longitudinal magnetism; whereas, if we place them without the spiral, they suffer no change. This experiment is due to M. Arago. Sir H. Davy, without being acquainted with it, has since succeeded in magnetizing small steel needles, by rubbing them transversely on a single conjunctive wire, or even without contact, by placing them at some distance from it. This process does not differ from the preceding, except in using the force of only one wire, a force which the spiral multiplies.

Since the electricity developed by the friction of our ordinary machines, differs in no other respect from that evolved from the voltaic apparatus, than that the former is retained and fixed, while the latter is in motion; we find that whenever we cause the electricity of our machines to flow in a continuous current, it has produced absolutely the same effects as the voltaic battery. The similarity, or rather the identity, of these two forms of electricity, is manifested likewise in the production of the electro-magnetic phenomena. This has been shewn by M. Arago, who transmitted along the spirals of M. Ampere, no longer the voltaic current, but a succession of very small sparks, drawn from a common electrical machine. Small steel needles, placed in the interior of these spirals, were thus magnetized in a few instants, and the direction of their magnetic polarity was found to be determined in reference to the surfaces charged with the resinous or vitreous electricity, precisely as happens with the copper and zinc poles of the voltaic apparatus. Sir H. Davy has also obtained similar results, by passing common electricity along a simple metallic wire, in the vicinity of which, small steel needles were placed at right angles to its length.

ART. VIII. *Description of a new Sinumbral Lamp: in a Letter to the Editor of the* Quarterly Journal of Science and the Arts.

SIR,

MAY I beg the favour of you to insert in the next Number of your *Journal*, the following brief notice and sketch of a new Shadowless Lamp, invented and manufactured by Mr. Thomas Quarrill, of Bell-court, Doctors' Commons.

I am, Sir, your constant reader,
and occasional contributor,
L.

A section of Mr. Quarrill's lamp is represented in Plate V, Fig. 2, from which it will be apparent that the oil reservoir is so shaped as to conform with the direction of right lines issuing from the brightest part of the flame, a portion of the light of which is thrown down by a small reflector upon the circular plate of ground glass, which fills the lower part of the lamp, and which is surrounded by the oil vessel. The chimney of the lamp is constructed as usual, and the whole is surmounted by a ground-glass light-distributor, so formed as to do away all shadow from any portion of the lamp, and at the same time not to offend the eye by any want of elegance in shape or dimension.

[We have received a drawing of the entire lamp which we have not thought it necessary to engrave, as the section above alluded to exhibits its material parts, and shows the peculiar excellence and advantages of Mr. Quarrill's construction.]

ART. IX. *Observations on the Solar Eclipse, September 7th*, 1820.

[Continued from p. 39.]

THE observations contained in the preceding Number were almost exclusively confined to details of the different appearances exhibited by the moon's surface. From the facts observed, it has been endeavoured to deduce some conclusions respecting the general forms, magnitudes, and relative positions, of its more prominent inequalities. Those circumstances in the present phenomenon, which appear to be more immediately connected with the question of a lunar atmosphere, still remain to be considered.

Whether the moon, like the earth, be surrounded by an aëriform medium, is an inquiry that has long engaged attention, and in which various modes of investigation have been pursued. Prior to the time of Halley, it does not appear that much attention, as regards this matter, had been paid to the observation of eclipses*. Subsequently, however, both in the original accounts of these phenomena, and in the more elaborate works on astronomical science, considerable importance, in relation to the above inquiry, attaches to certain appearances, observed externally round the margin, as also within the circumference of the moon while visible on the sun's disc. In the instance now under review, several of these appearances were observed. Instead, therefore, of merely enumerating isolated, and in themselves, perhaps, unimportant, facts, it is proposed in the following remarks, to view them in connexion with the subject at large, and as compared with the results of similar observations in preceding eclipses, arranging the different circumstances in the order of their occurrence.

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* Kepler, about the commencement of the sixteenth century, had stated, in a general manner, that several appearances in eclipses of the sun, seemed to indicate the presence of matter of extreme tenuity surrounding the moon (*vide Astron., pars Optica*). There are also a few remarks of the solar eclipse of 1706, tending to establish the same opinion (Flamstead, Cassini, *Phil. Trans.*, for that year, and *Mem. de l'Acad. de Scien.*, 1706, p. 347). Scarcely any thing authentic, however, was known previous to the publication of Dr. Halley's *Memoir* on the total eclipse of 1715. A circumstance there mentioned still further confirms this. No eclipse of the sun had been observed in London from the year 1140, although the path of the umbra on several occasions must have crossed that meridian.

From the original accounts* of the more remarkable solar eclipses, since the period above-mentioned, flashes of light darting in various directions, and from different parts of the moon's darkened hemisphere, appear to have been frequently observed. In general, these appearances seem to have been visible about the time of greatest obscuration only. "During the whole time of the *total eclipse*, (says Dr. Halley, in describing that of 1715). I kept my telescope constantly fixed on the moon, and I saw *perpetual flashes of light*, which seemed for a moment to dart out from behind the moon, now here, now there, on all sides, but more especially on the western side, a little before the emersion†." In his *Memoir* on the eclipse of 1748, nearly the same account is given by M. de Thury:—

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* The eclipses here referred to are chiefly those of 1715, 1724, 1736-7, and 1748, of which the principal accounts are by Halley, Loville, Cassini, Maraldi, M'Laurin, Le Monnier, De L'isle, *&c. Phil. Trans.* and *Mem. de l'Acad. de Sciences.* Of that in 1724, there is no account by an English astronomer, notwithstanding, as appears from the map which accompanies Le Monnier's *Memoir sur les Eclipses totale du Soleil*, the path of the shadow must have traversed great part of Britain, (*Mem. de l'Acad. de Scien.* 1781, p. 243). Of the annular eclipse of 1764, the accounts are few and uninteresting. Though great preparations were made by the French astronomers, Card. de Lugnes seems to have been the only person who witnessed the formation of the annulus, by a momentary opening of the clouds. (*Mem. de l'Acad., &c.*, 1764.)

† *Phil. Trans.*, No. 343, vol. 29, p. 248.

" J'ai decouvert vers *le milieu* de l'eclipse, sur la surface de la lune, comprise entre les cornes du soleil, des *rayons* de lumiere rouge*," *&c.*

A general conclusion from these appearances is, that " they are flashes of lightning, such as a spectator in the moon would see in our earth if its whole hemisphere were interposed between him and the sun†. Hence it is inferred, that " the moon has an atmosphere similar to ours, wherein vapours and exhalation may be supported, and furnish materials for clouds, storms, and tempests‡."

Of these phenomena, another explanation has been given, in which it is assumed, that cavities in the body of the moon, acting as concave mirrors, reflect the solar light obliquely to the eye of the observer, and that the progressive motion of the planet in its orbit, by constantly changing the position of the reflecting surfaces, in relation to the line of vision, causes the rays thus suddenly turned from their former direction, to appear in momentary streams or coruscations of light§.

Without insisting on the numerous objections which are opposed to the former of these hypotheses, such as the casual nature of the occurrence on which it rests, the small space over which its influence could be visible, the weakness of the illumination which it could produce, as compared with the extent, duration, and brilliancy of the effects observed—the very cause assumed would remove the possibility of viewing the effect from which it is inferred. Granting that such an atmosphere as is contended, does actually surround the moon, an observer on the earth would not, in that case, be able to discern the lightning, since the whole mass of vapour must necessarily intervene, and conceal it from view; as, by the laws which regulate its motions, the flash would pass between the surface of the moon and the adjacent clouds, in the lowest stratum of this supposed atmosphere.

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* *Mem. de l'Acad. des Sciences,* 1748, page 56.

† *Phil. Trans.*, as quoted above.

‡ Long's *Astronomy.*

§ *Astron.* de La Lande; M. De I'sle, *Memoires pour servir à l'Hist. de l'Astron.*, 1733, page 248; and *Mem. de l'Acad. des Sciences.*

That the appearances in question, therefore, are simply modifications of the solar rays, seems extremely probable. To the principle of the second theory, however, it is justly objected, that from the known properties of light, and the laws of vision, no form or position of cavities in a spherical body, or indeed of any figure interposed between the spectator and the source of illumination, could by reflection render the rays of light from that source visible on the opposite dark surface of the intervening mass.

To compare these different opinions with the inferences from observation. From an early period in the instance before us, appearances resembling those above alluded to might be distinguished. As the progress of obscuration advanced, the flashes became gradually more distinct, exhibiting the greatest intensity of light, immediately preceding and subsequent to the ecliptic conjunction. From this to within 12′ or 15′ of the egress, when they ceased entirely, a similar gradation, but inverted, was observed, these luminous streams appearing more and more faint, as the termination of the eclipse approached. This is merely to be considered as a general statement, implying that coruscations of comparatively the strongest brilliancy were most frequent about the time of greatest immersion; but, as has already been observed*, no regularity in the individual alternations could be perceived. These radiations were not confined to any particular part of the lunar circumference; they were, however, *cæteris paribus*, more rarely visible on that division of the lower or western limb, where the largest portion of the sun's disc remained, at the time, unobscured. Their splendour was often considerable, more especially when viewed near the cusps; but in no situation so bright as not to have easily escaped the notice of an observer whose researches were directed to other objects. Sometimes they appeared to shoot from behind the moon, at others to play along the margin, and not unfrequently to dart in various directions, and to different distances towards the centre of the moon's disc.

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* *Journal of Art and Science*, No. 21, page 28.

Some time had elapsed before any indications of a general law could be discovered to regulate these effects. It was at length invariably observed, that wherever the inequalities of the lunar circumference were most conspicuous, flashes of light appeared to proceed most frequently from that quarter. Also, that the greater the space in the darkened hemisphere, over which they seemed to traverse, the more distinct they shewed, and the nearer did their colour approximate, from a very dilute purplish tint, to a red or dusky light.

These two facts seem to afford an easy solution of the phenomena in question. From the former it sufficiently appears that the primary cause is to be sought for in the inequalities of the moon's periphery. The solar rays, then, which were intercepted by the circumference falling upon the declining sides of the lunar mountains, would be variously reflected, not merely from the angular direction of the incident rays, but also as regards the position of the reflecting surfaces in relation to the whole mass,—when reflected from the side directly exposed to the sun, coruscations of thin light would appear to radiate from behind the moon—as the reflecting point was situate nearer the anterior surface, they would tend with proportional inclinations, towards the interior of the disc. The second fact is rather a necessary consequence, than a cause of these effects. If these luminous streams or flashes proceeded from the reflection of the sun's light, it is obvious that the direction of the reflected rays would materially influence their brightness. When the path of the reflected rays traversed any considerable portion of the moon's dark surface, they would produce a flash, necessarily more brilliant than in a direction nearer the circumference, both from the greater contrast of the surrounding shade, and as being more remote from the splendour of the sun's unobscured segment. The greater the darkness, therefore, the more distinct they would appear; and hence they differed in this respect, according to the different phases in which they were observed. Hence, also, although by careful observations they might probably have been discovered in nearly all the phases, as in the present instance; yet, in the greater number of eclipses, they have been observed during the continuance of greatest immersion only. From similar causes, appearances resembling the above have been most conspicuous in total eclipses, and might then, from a casual view, have been easily mistaken for flashes of lightning. As an additional proof, it may be worthy of remark that after attentively watching their progress, by taking into the field of the telescope that part of the disc only where their appearance might be anticipated, these radiations were always found to come from the periphery, never from the interior of the moon's orb.

According to the quantity of the reflected rays, and their being occasionally, from position, longer exposed to the view of the observer; those broader and more permanent streams of light, which appear to have been visible in almost all eclipses of this nature, would be produced. One of these, which occurred nearly at the moment of conjunction, on the moon's western limb, as illustrative of the situation of the lunar mountains, has already been described.

In reference to the present inquiry, it may be further remarked, that no haziness, or melting of the light into the surrounding shade, could be observed; on the contrary, the line of demarcation was harshly traced, and the confines perfectly distinct, which would hardly have been the case, had the illuminated surface been surrounded by a medium capable of refracting the solar light. The reflected light, certainly, was less vivid at a distance, from the circumference; but this evidently arose from its being divided into unequal streams by a ridge of mountains. The outline of this ridge, as also that of the periphery, were likewise very distinctly marked, without the slightest appearance of an external and more faint illumination, such as would have been produced by any attenuated luminous matter surrounding them. (See Fig. 4, Plate III, of last Number.)

Another class of phenomena, from which considerable diversity of opinion has originated, is that of detached masses, or spots of light, of various extent and brilliancy, sometimes during former eclipses, observed within the moon's disc. The appearances in question, however, seem so little different from those now discussed, that they may be considered under the same head, a deviation from the order of time which it was proposed to follow, being the only inconvenience attending this arrangement.

At 2h. 51′ 24″, in the present eclipse, one of these luminous spots was observed. Its position was considerably within the margin of the limb, and distant from the eastern cusp, about $\frac{1}{3}$ of that portion of the moon's circumference then visible on the solar disc. It remained perfectly distinct for nearly $1\frac{1}{2}$ minutes, exhibiting with little variation in size, one uniform appearance of a large irregular macula of reddish light, and when brightest, equal in splendour to a star of the 4th or 5th magnitude. The outline on the upper and lower sides, was well defined with deep angular curvatures, especially in the former. From the point of apparent intersection of the two peripheries, in the moon's eastern limb, a stream of paler light could be traced in a direction nearly parallel to the circumference, till it joined this spot. From its opposite side, a similar stream, but of still fainter colour, extended almost in a line with the point of contact in the western limb, to about one fourth the distance between them. (See Fig. 5. Plate III.)

Appearances similar to the above, not unfrequently occur, in the accounts of preceding eclipses. Thus, M'Laurin, in describing the one of 1737, states, that "before the annulus was complete, a remarkable point or speck of pale light appeared near the middle of the part of the moon's circumference that was not yet come upon the disc of the sun: and a gleam of light, more faint than that point, seemed to extend from it to each horn." He adds, "I did not mark the precise time, when I first perceived it, but am satisfied it could hardly be less than $\frac{1}{4}$ of a minute before the annular appearance began*". In that of 1748, in like manner there was seen by Short, "about the middle of the eclipse, a remarkably large spot of light, of an irregular figure and of a considerable brightness, about 7′ or 8′ within the limb of the moon." It is rather singular, that the Earl of Morton, who was making observations with a reflector, in the apartment immediately adjoining, could not perceive this appearance. "I lost this light" continues Mr. Short, "several times: whether this was owing to shutting my eyes in order to relieve them, I cannot tell; when I first observed it, I called to my Lord Morton, but he could not perceive it[*]." Many other instances might be adduced similar to those now quoted[†]. On former occasions, these appearances seem to have been almost invariably observed about the middle of the eclipse; that, in the present case occurred towards the commencement of the last quarter: the only circumstance, however, materially different, is, that in several, the streams of faint light were not seen, or at least are not mentioned.

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* *Phil. Trans.* vol. lx, p. 181.

To explain these phenomena, various hypotheses have been framed[‡]. Among the proofs of an atmosphere, the supposed existence of lunar volcanos, as almost decisive of the question, naturally holds a prominent place. The preceding appearances accordingly are made to favour that opinion, and these isolated sparks of light, have been attributed to volcanic effects. The evanescent nature of these appearances, their apparent great extent, their having been hitherto visible, at certain phases only, the lines of fainter light proceeding from them, are circumstances seemingly incompatible with such an origin. The solutions opposed to this opinion are, however, not less liable to objection. Since the appearances in question, have been successively ascribed, to perforations in the body of the moon, to igneous vapours in the atmosphere floating across the field of vision, and to reflections of the solar rays from the surface of the earth, which impinging on the summits of the lunar mountains, are again transmitted to the eye of the observer. The last is the supposition least exceptionable, yet is obviously far from accounting for all the effects. Besides, were this the real cause, the light would be more universally diffused, as is seen in the new moon, when the whole unenlightened part of the orb is faintly illuminated by rays reflected in the same manner as is here supposed.

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[*] *Phil. Trans.* vol. xlv, p. 588.

[†] *Phil. Trans.* vol. xxxviii, p. 332. vol. xli, p. 94, &c. *Mem. de l'Acad. de Sciences.*

[‡] See Schroter's *Selenotopographische Fragmente*, &c.

To apply the facts already discussed, to the explanation of the present phenomenon, let it be observed that the lunar mountains are probably arranged in chains of great extent*. That one instance has been adduced in which reflections of the sun's rays were separated into different streams by ridges apparently in this situation; and that, during the whole eclipse, the luminous appearances previously mentioned, were most conspicuous near the cusps, simply because in that position the inequalities of the periphery were most advantageously placed for intercepting, and reflecting the solar rays, in directions traversing the moon's disc. One of the larger and more permanent streams of reflective light, then, passing behind one of these mountainous ridges, in a line inclining to parallelism with the moon's horizontal diameter, would either be altogether invisible, or but faintly seen, according to the degree of its elevation. If the ridge were continuous throughout its whole extent, no new effect would follow; but if a discontinuity of any considerable length occurred, the illumination becoming then more diffused, would present the appearance of an irregular lucid spot, varying in extent, form and brilliancy, according to the different modifying causes. The probable cause has been pointed out why these streams generally appear to proceed from one of the cusps; striking on the elevations at the other extremity of the opening, the rays would again be reflected, from a well known law in optics, in a direction tending to the opposite cusp, in proportion as the point was nearer the centre of the intervening circumference. Exactly the same effects would follow, if instead of being diffused through an interruption, it be supposed that the light is reflected from a more elevated portion of the ridge. The former supposition better explains those instances, in which, like the present, the illumined spot lies in the lower limb; the latter is more applicable to such, as, like that mentioned by M'Laurin, have it in the upper margin. It is obvious also, that when the direction of the reflected rays was so depressed as not to appear above the summits of the ridge, a solitary speck of light, such as seen by Short, would alone be observed.

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* *Journal of Arts and Sciences*, No. 21, p. 38-9.

In the preceding investigation of one class of phenomena from which the agency of a lunar atmosphere is inferred, no other principles of discussion, except those derived from actual observation, have been admitted; the inequalities of the moon's surface, and reflection of the solar rays. That the effects described, are at least intimately connected with these causes, still further appears from the fact, that in those eclipses in which the former have not been clearly perceived, these luminous streams, flashes,and spots have not been observed. Thus, Weidler states, that on one occasion, although very distinctly marked, the outline of the moon's disc appeared without any elevations or depressions, such as he had formerly seen, and even attempted to measure; as also unaccompanied by any of those indications of an atmosphere which he had previously noticed. "Cæterum lunæ discus, sub sole, peripheriam *accurate terminatam,* absque ulla *inequalitate* nec non faciem nigerrimam ostendit. *Nullum* quoque *atmospherice* orbi lunæ insidentis *vestigium* potuit deprehendi*". On the whole, therefore, it may be concluded, that from the appearances now described, no proofs of a lunar atmosphere can be deduced.

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* *Phil. Trans.* vol. xli. p. 94, Weidler appears to have paid great attention to the observation of solar eclipses, with a view to establish the hypothesis of a lunar atmosphere. He has published details of several, of considerable magnitude, in which he ascribes various streams of light, *&c.* to atmospheric effects, and adds, that he not only saw them, but "*guadens vidit.*" On all these occasions the mountains were very conspicuous, the depth of one valley is estimated at $\frac{1}{200}$ of a diameter.

Among the phenomena, which have been considered as most directly indicating the presence of this atmosphere, are to be reckoned those luminous circles or halos, which, at the time of complete immersion, in total and annular eclipses have been observed concentric with, and immediately surrounding the moon's circumference. No appearance of this nature could be observed on the present occasion; about 2′ indeed after the conjunction, as determined by previous calculation, the dark and hitherto strongly defined outline of the periphery seemed less distinct, but without the slightest appearance of any extraneous matter.

From the original observations, it appears, that these luminous rings seemed to break out from behind the moon, varying in breadth from a digit to $\frac{1}{10}$ of a diameter. They were brightest near the body of the moon, and of a pale, or, as it is expressed, "pearly" light, gradually diminishing in splendour as the distance increased, and apparently "terminated by the extreme rarity of the matter of which they were composed[*]."

In order to account for these appearances, three different theories have been proposed. The hypothesis of Cassini, in which it was assumed that they proceed from the effects not of a lunar, but of a solar, atmosphere, appears never to have been generally adopted. The principles of the other two are thus briefly stated by Le Monnier. "Ou bien la lune est environnée d'une atmosphere tres deliée, et dont la matiere est fort

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[*] *Phil. Trans. Mem. des l'Acad. des Sciences. Mem. de Berlin, (Euler). Mem. de l' Institut. (Lalande).* There is a remarkable coincidence in the estimates of the breadth of these circular areas. Flamstead, Halley, Cassini, *&c.*, stating it as in the text, and others even greater. M. de Ihering's measurement it is not now easy exactly to ascertain. "Un filet de luminière sembloit masquer le disc de la lune, et qui s' etendoit a une distance de cornese gal a peu pres a l'espace compris entre trois fils de micrometre." (*Mem. de l' Acad. des Sciences, an.* 1748, p. 56.) The colour of these rings does not appear to have been invariably a pale white, but in some instances extremely brilliant: thus Cassini mentions, that at Zurich, "Le sollei fut couvert pendant quatre minutes, et le bord de la lune paroissoit comme un anneau d'or. *Mem. de l' Acad. &c., an.* 1706, p. 347.

rare; ou ce qui revient au meme les rayons du soleil qui rasent la lune, souffrent une refraction d'autrant plus grand qu'ils s'approchent a ce corps, et par consequent, s'inclinent vers l'axe du cone d'ombre au lieu de parvenir en ligne droite du soleil jusqu'à notre œil*."

It is not intended to enter into any discussion of the various details connected with this part of the subject, as being foreign to the present design; no opportunity having occurred of comparing former opinions with the results of actual observation. It may be necessary, however, briefly to shew, how far the conclusions legitimately derived from the two principal theories are decisive of the general question. In one respect their principles are identical; both assuming that these luminous rings arise from the rays of light being inclined towards the axis of the conical shadow; they differ only as to the primary cause in which this defection originates. In the one it is maintained, that the moon being surrounded by an atmosphere of the same nature as ours, its effects in refracting the solar rays must also be similar, and that these illumined circular areas seen round the margin, to the extent of $\frac{1}{12}$, or in some instances $\frac{1}{10}$, of the moon's diameter, are thus produced. Now, as the refractive properties of homogenous media depend on their densities; and, since, from the diminution of gravity, the density of the earth's atmosphere is to that of the moon's nearly as 408 : 139†, their refraction must be in the same proportion. The mean elevation, also, at which the atmosphere of the former is capable of reflecting the twilight, is about $\frac{1}{44}$ of a diameter of the latter, therefore, as $408 : 139 :: \overset{\text{char.}}{\frac{1}{44}} : \overset{\text{diur.}}{\frac{1}{29}}$ the altitude of lunar refraction on the highest estimate, which, however, is not equal to one-tenth of the extent of the appearances in question. But as

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[*] *Mem. de l' Acad. des Sciences, an.* 1765. See also *Mem. de Berlin,* 1748, p. 103. Where Euler adopts the theory of a lunar atmosphere, and *Mem. pour servir a l' Hist. del Astron.* 1733, p. 243—50, &c. in which De I'sle supports the hypothesis of inflection.

[†] *Newt. Princip. Mathem.* From an erratum in page 34 of last Number, gravity at the surface of the moon is said to be diminished "one-third" instead of *to* "one-third" "nearly."

clouds and vapours are never found above the highest mountains on the earth, none of which exceed $\frac{1}{436}$ of the moon's diameter, it is to be concluded that the grosser part of our atmosphere, capable of producing any *sensible* refraction of the rays of light, does not extend beyond that elevation. Consequently, as 408 : 139 : : $\overset{\text{dia.}}{\frac{1}{436}} : \overset{\text{dia.}}{\frac{1}{1279}}$, which, on the same principle of analogy as is recognised in the theory itself, gives a very near approximation to the true extent of lunar atmospheric refraction ; a quantity, nevertheless so small as not to subtend an angle of 1″, and which does not exceed one mile in perpendicular elevation, whereas the circles of what is termed refracted light, appear to have extended at least 160 miles beyond the moon's surface.

The second theory is found on a property of matter, by which it deflects, or attracts towards the perpendicular, the rays of light which pass very near its surface. From experiment, however, the quantity of light thus deflected is so inconsiderable as to be apparently inadequate to the production of the effects in question. If the observations of Maraldi are to be considered as accurate, the body of the moon must be regarded as even altogether destitute of this property of terrestrial matter. But we are not left to doubtful speculation on this subject. Since it has been determined that the aberration of the rays produced by the attraction of the moon's surface does not exceed 3.5″, a quantity which certainly appears insufficient to account for the phenomena above described*. Such are the limits prescribed, both by analogy and experiment, to the operation of those causes, which are assumed as perspectively producing the effects in question. That they cannot far exceed these limits also appears, for if the inflection of the solar rays, whether effected by atmospheric refraction, or by the attraction of the moon's body, exceeded 7″, the apex of the shadow would fall within the mean distance of the node, and consequently a solar eclipse could never happen. The nature of these luminous rings, therefore, seems altogether inexplicable; at least, the theories which have hitherto been proposed on that subject, leave the question of a lunar atmosphere undecided.

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* M. du Sejour trouva qu'il falloit faire l'inflexion (de rayons que rasent les bords de la lune) d'environ $3\frac{1}{2}''$. M. Mechain, et M. Lexelle ont trouvé le même resultat.—*La Lande Astron* tom. ii. p. 445. *Sejour. Traité des Mouvem.*, &c.

The evolution of greater light and heat from particular portions of the unobscured part of the sun's disc, is one of the most interesting phenomena connected with the subject of eclipses, and at the same time appears one of the most inexplicable. Dr. Halley is the first, and indeed the only observer, who distinctly noted the circumstance, and seems to have remarked it, about the time of greatest obscuration only. On the present occasion, however, from the obscuration of 5½ or 6 digits, to the same phase in the egress, it was frequently experienced that more light and heat were transmitted from the western than from the eastern divisions of the sun's disc. This effect was very sensibly felt on the eye and face, when the telescope was pointed in immediate succession, to the respective portions of the unobscured segment, taking care to move it across the moon's dark surface, or below the sun's lower limb; and to admit into the field, only a small part of the enlightened disc near each cusp. Dr. Halley's statement is somewhat different. " When the first part of the sun remained on his east side, it grew very faint, and was easily supportable to the naked eye even through the telescope, for above a minute of time before the total darkness; whereas, on the contrary, the eye could not endure the splendour of the emerging beams through the telescope even for a moment. He ascribes this to two causes, " the dilatation of the pupil during the darkness, which before had been contracted by looking on the sun," and the eastern parts of the lunar atmosphere being replete with vapours raised from a surface exposed during thirty days to the rays of the sun, and from the opposite cause the western parts would be pure and transparent. Neither of these seems a satisfactory explanation; the fact indeed scarce admits of one, but if a conjecture may be hazarded, the cause is, perhaps, to be looked for in some property of the sun itself*.

At the termination of the eclipse, although a small triangular portion of the moon's periphery could be seen, when every other part had passed off the solar disc, not the least appearance of refraction, or any indication of an atmosphere could be perceived. For some moments after egress, the moon totally disappeared, but the observation was anxiously continued, in hopes of realizing M'De Isle's suggestion. About 2″ from the sun's margin, the finely attenuated film of pale light was descried, which gradually increasing, at length appeared to extend along nearly $\frac{1}{8}$ of the moon's circumference, exhibiting at the same time considerable breadth, much greater, indeed, than could have been supposed considering its extreme proximity to the sun. The colour of the illuminated surface was similar to, but more faint than, that of the moon, when sometimes seen during the day. The light towards the extreme points seemed to disappear by degrees; at the centre it shewed more acutely defined, the circular outline, nearest the sun, was perfectly distinct; the appearance, however, was so transient, that a general description only can be given.

The straight line joining the extremities of the enlightened segment, would have been nearly at right angles to the path of the centre; and the illumination evidently was such as would arise from the effects of the solar rays falling on a spherical body, unconnected with any atmospheric medium. (Plate III. Fig. 6)

At the time of greatest obscuration, the diminution of light, although considerable, was by no means so great as had been anticipated. A mild agreeable lustre was diffused over the nearer objects, and it was only in the deep blue tones of the back ground that one could recognise

> ———A faint erroneous ray,
> Glanced from the imperfect surfaces of things,
> Fling half an image on the straining eye.

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* M. Le Monnier seems to have misunderstood this description of Dr. Halley's, and to have taken for indications of atmosphere, what the latter has ascribed to the effects of the lunar mountains.—See *Mem. de l'Acad. des Sciences*, 1781, p. 252.

ART. X. *On the Divisibility of Matter.*

To the EDITOR *of the Journal of Science and the Arts.*

SIR,

THE following calculations, on the extent to which the divisibility of matter may be carried in the particular instance which I have endeavoured to illustrate, have never, to my knowledge, been yet offered to the public; and as they may be the means of inducing others, more adequate thereto, to take up the subject, I have ventured to request their insertion in your *Journal*, should you deem them admissible: entreating the candour of your readers in the perusal of the present crude statement,

I remain, Sir,
Your obedient servant,
S——.

June 14, 1821.

It has been calculated by Mr. Boyle, I believe, that fifty square inches of leaf gold weigh only one grain; and that an inch in length may be divided into two hundred parts, each visible to the naked eye. Consequently, each square inch will contain forty thousand such parts: for 200 × 200 = 40.000, and this multiplied by the fifty square inches, will make two million visible pieces, into which a single grain of gold may be divided; this, however, does not come near the ideas of an eminent professor, who has recently asserted, that gold, in the gilding of silver wire, may be reduced to the thinness of a twenty-millionth part of an inch; and, as he illustrates it, will bear only the same relation to an inch, as the thickness of a sheet of paper would to a mile in length. If this be the fact, and we allow only 200 visible parts in the inch, it follows that each 200th part, as above, may be further divided into 100,000 other parts, so that a single grain of gold may be capable of being divided into one hundred thousand times two millions, or

200,000,000,000, which, though approaching to almost infinite divisibility (at least, according to our limited ideas,) yet we must all feel to be mathemically true; and even these instances appear to fall far short of the extent to which the operations of nature carry the actual divisibility of matter, as exhibited more particularly in the minute particles of odoriferous bodies, constantly filling surrounding spaces to a considerable distance, without any perceptible diminution; and perhaps still more so, in the wonderful formation of the animal kingdom, as more peculiarly displayed in the minute (in many instances, invisible,) insect tribe, each of which possessing attributes of the larger animals, as muscles, circulation of the blood, *&c.*, must very far surpass any ideas which the human mind can form on the subject; and yet it is possible, that even these may be still further surpassed by the divisibility of the particles of light. Let us take the well-known effect of a lighted candle, which may be seen at the distance of two miles, and probably further; in this instance, light is diffused almost instantaneously, and that without any sensible diminution of weight, throughout a circle, whose diameter is four miles; or rather, supposing the light placed upon a plane, it will extend or diffuse light throughout a hemisphere of that dimension, whose centre is the flame of the candle. During the process of combustion, the light, according to *Richter's Theory*, proceeds from the combustible body; however this may be, it should appear evident that, in the production of light (from a candle, for instance,) a certain quantity of matter, either combined or uncombined, is diffused through a given space in a given time. Let it be allowed, that a candle, weighing four ounces, will burn, or diffuse light, for six hours; and that, during that period, it fills unceasingly a hemisphere, whose radius is two miles, or 126,720 inches, containing by computation, if I am correct, 4,261,820,184,605,491$\frac{2}{10}$ square inches. Now each square inch was found capable of being divided into 40,000 visible parts; consequently, the hemisphere contains 170,472, 807,384,219,648,000 parts visible to the naked eye, and which are unceasingly illuminated by the diffusion of light from a certain portion of matter (combined or uncombined), weighing four ounces, for the space of six hours. Now say, four ounces is 1,920 grains, which, for six hours, will give about $\frac{1}{12}$ grain per moment; this $\frac{1}{12}$ grain of matter is thus found to be instantaneously divided into 170,472,807,384,219,648,000 visible parts; and consequently each single grain into twelve times that amount which is 2,045,673,688,610,635,776,000, or upwards of two thousand millions of millions of millions. Now a grain of gold was found divisible into two millions of such parts, it therefore follows, that the divisibility of gold to light, contained in the inflammable matter, supposing the foregoing to be a correct statement, is as 1 to 1,022,836,844,305,317$\frac{8}{10}$, or as one to above one thousand millions of millions; and even this may be comparatively trifling, to the probable diffusion of the solar light.

Our limited powers of comprehension are very inadequate to form just conceptions of infinity, and the preceding view of the divisibility of matter, may perhaps tend, in some degree, to elucidate a subject which, to the generality of those who are not in the habit of studying the power of numbers, would appear possibly as incredible as the immensity of space exhibited in the starry heavens, as laid down in astronomical calculations; for when we say, that the distance of certain heavenly bodies is millions of millions of miles; or, that a single grain of matter may be divided into millions of millions of visible parts, a smile of scepticism would with some be the only result of an endeavour to enforce the truth thereof. May it not serve to familiarize the subject to the inquiring mind, to observe, that we may suppose it sufficiently easy to comprehend that the space of the tenth part of an inch may be divided into twenty parts, which is two hundred to the inch. Allow this, and we readily prove that each square inch contains forty thousand, and the solid inch, eight millions such parts; yet to assert, that a solid inch of matter may be divided into only eight millions,

Mr. Daniell's Pyrometer.

Fig. 1.

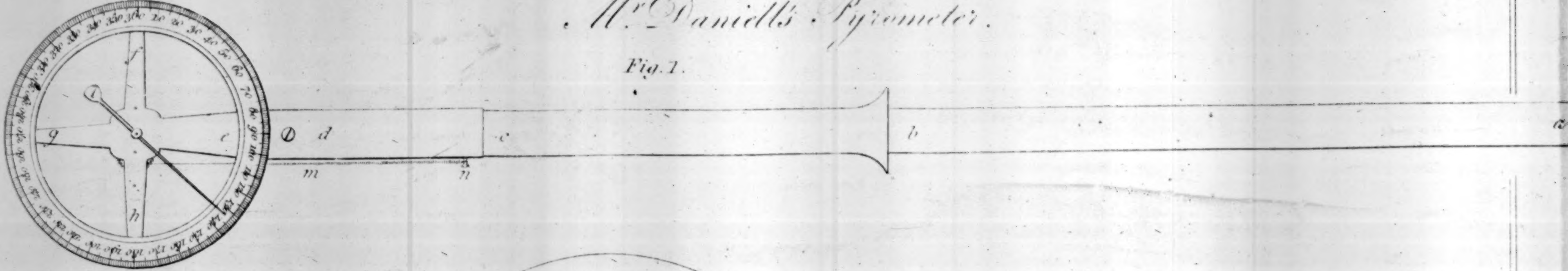
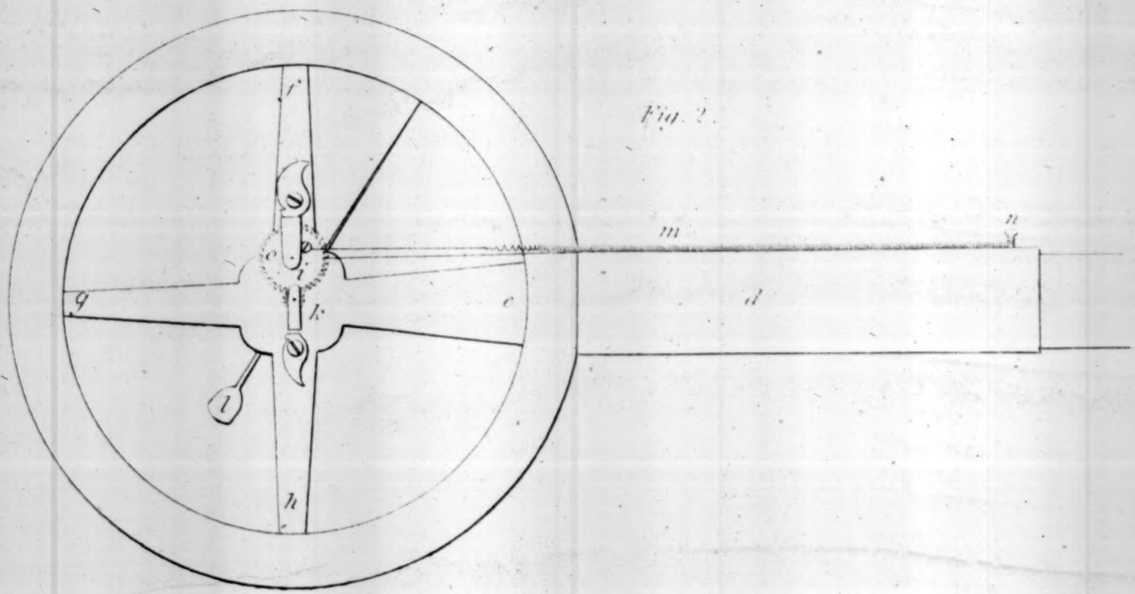
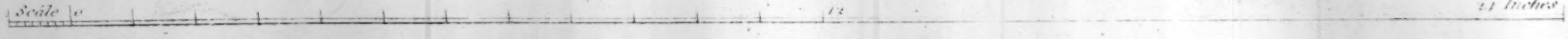


Fig. 2.





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would appear to the mere superficial observer, as beyond credulity, though it is capable of actual and practical proof, and even, as in the case of gilt silver wire, to an extent immensely beyond. Why, therefore, doubt the deductions made upon principles that cannot err, merely because they are beyond our present ideas of possibility? As well might we deny the existence of the Creator, because his works transcend our limited powers of conception. And may we not, from this deduce a powerful argument in favour of the truths of revealed religion: for if each individual is to doubt of every thing that exceeds his own peculiar ideas of probability, at what point shall incredulity find a barrier? That immensity and divisibility (and who shall say where they may find a limit?) approach the confines of infinity, must appear evident to every one who has seriously contemplated the results; and if the preceding may, in any way, tend to illustrate the subject, or induce others to lend their aid thereto, the end of the present attempt will be accomplished.

ART. XI. *On a New Pyrometer.* *By* J. F. DANIELL, Esq. F.R.S. and M.R.I.

[With a Plate.]

IT would be needless to preface much upon the utility of an instrument to measure the higher degrees of heat, as nothing in science has been more eagerly desired, and nothing, it is generally allowed, would tend more to the perfection of many of the arts. The difficulties which oppose themselves in practice to any contrivance for this purpose, are best appreciated from the knowledge of the fact, that but one attempt has ever been made, with any degree of success, to solve so interesting a problem. The late Mr. Wedgwood invented an instrument with this view, founded on the principle that clay contracts in its dimensions in proportion to the intensity of the heat to which it is exposed. His experiments and results are alone referred to in books of science upon this subject, but the pyrometer itself has long fallen into disuse, partly from the difficulty of obtaining clay-pieces of an uniform composition, but chiefly from the discovery that a long continuance of a lower degree of heat produces the same effect of contraction as the shorter continuance of a higher degree.

Being struck with the importance of the subject, I have lately bestowed much of my time, and made many fruitless endeavours to attain this desirable object. At length, however, I flatter myself that I have been fortunate enough to combine an instrument extremely simple in its construction, very manageable in its use, not liable to injury, when injured easily repaired, and which will extend the scale of the thermometer, at least to the fusing point of cast iron. Its sensibility also is very great, considered with regard to the extensive range which it is destined to measure. A change of about seven degrees of Fahrenheit's scale is distinctly indicated by it, while, on the other hand, every degree of Wedgwood's pyrometer was calculated to be equivalent to 130° of the same thermometer.

The results which I have obtained with this instrument differ, unfortunately, very widely from those of Mr. Wedgwood, but I shall, as I proceed, state my reasons for believing that mine are the more accurate of the two. I shall not enumerate the different steps by which I proceeded, but shall at once describe the pyrometer in the most perfect state which my hitherto limited experience of its use enables me to suggest.

Plate VI., fig. 1, represents the instrument drawn to the scale at the side of the plate. Fig. 2 represents a part of the same of half the real dimensions. The tube *a b c* is made of black-lead earthen ware, and the shoulder in its centre is moulded in its construction. The extremity *a* is close, and the extremity *c* open; *d* is a ferule of brass into which the end of the black-lead tube is accurately fitted, and to which the scale *e f g h* is attached. In the inside of the tube *a b c* lying upon it, and extending to *b* is a bar of platinum 10.2 inches long, and 0.14 of an inch in diameter. It is immovably fixed at *a* by a nut and screw of the same metal on the outside, and a pin or shoulder on the inside. It is likewise confined to its place at *b* by a small perforated plate of platinum through which it passes. From its end *b*, proceeds a fine platinum wire of about $\frac{1}{100}$ of an inch diameter, and coming out of the tube at *d* passes two or three times round the axis of the wheel *i*, fixed on the back of the scale *e f g h*, and represented at fig. 2. It is then bent back and attached to the extremity of a slight spring *m n*, which is stretched on the outside of the brass ferule, and fixed by a pin at *n*. The wire is thus kept extended by the action of the spring. The axis of the wheel *i* is 0.062 of an inch in diameter, and the wheel itself is toothed and plays into the teeth of another smaller wheel *k*. This smaller wheel is $\frac{1}{3}$ the diameter of the larger, and carries on its circumference $\frac{1}{3}$ the number of teeth. To its axis, which passes through the centre of the scale *e f g h*, is attached the index *l*. Now the theory of this combination is, that any alteration of the relative lengths of the metal wires and earthen tube will cause the wheel *i* to move from the action of the spring *m n*, which motion will be multiplied three times by the wheel *k*, and measured by the index *l*. The scale is divided into 360°. Instead of passing the fine platinum wire round the axis of the wheel it has been found better in practice to attach a short silken thread to its extremity, and pass that round and fix it to the spring. The dimensions, which I have stated above, may, of course, be varied to suit different purposes. Nothing depends upon their nice adjustment, or upon intricate calculation. The value of the degrees, it will be seen in the sequel, is determined for each instrument by two fixed points in a manner perfectly analogous to the graduation of a thermometer.

If the extremity of the instrument *a b* be now gently heated the index will be seen to move forward with a gradual and very equal motion, and by careful cooling, will return as gradually from the point from which it started. The accession of heat causes the metal bar and wire to expand more than the earthen tube; the consequence is, that the action of the spring always keeping the wire tight draws the wheel round. In cooling the metal again contracts, and restores the spring to its former degree of tension.

If instead of heating the instrument gradually, it be plunged at once into a brisk fire up to the shoulder *b*, the index will at first move back some 10° or 20°: it will then become stationary for a short interval and afterwards move rapidly forward. The reason of this is that the sudden application of a high heat causes the tube to expand before its effect is felt by the included bar, the consequence is that the bar becomes relatively shorter, and the effect of contraction is produced upon the wheels; but directly that the influence of the heat reaches the metal, it rapidly overtakes the counter expansion of the tube, and the index immediately moves forward to the point which it would have attained if it had been gradually heated. The reverse of this takes place if it be suddenly taken from a high temperature into the cold air. This is one beautiful testimony of the delicacy and accuracy of the instrument. It is well known that an analogous effect is produced upon a thermometer under similar circumstances; if its bulb be placed suddenly in the flame of a candle, or it be otherwise suddenly heated, the mercury will appear to fall in the tube, that is to say, the expansion of the glass momentarily exceeds that of the metal.

After having ascertained that the effect of the combination was such as I had anticipated, and that the index moved forward regularly in proportion to the heat applied, and returned in cooling to the point from which it set out, my next object was to ascertain, if this effect were perfectly equable, and to obtain the value of the degrees, if possible, in degrees of the thermometer.

For this purpose I procured a cast-iron case, one foot in length, two inches wide, and two inches and a half deep, in which was a partition one inch from the end. In the centre of this end, and in the partition, were round holes which just admitted the stem of the pyrometer. The instrument was passed through these holes up to the shoulder, the small division of the case was then tightly stuffed with tow and lute, and the larger division was filled with mercury. The whole length of the platinum bar was thus immersed in that metal. The apparatus so arranged, with the needle pointing to 0°, and the temperature of the air being about 60°, was placed over two Argand lamps, or a small furnace, and gradually heated. The index of the pyrometer moved slowly and steadily forward from 0, and when it had reached 85° the mercury boiled rapidly. It was kept in a state of ebullition for half an hour, and the index remained stationary at that point. The strong concussions, indeed, of the boiling metal caused the needle to vibrate, but its motion was confined between the degrees of 83 and 85. Now, if we assume the boiling point of mercury, under the pressure of the atmosphere, to be 656° of Fahrenheit's scale, as we are justified in doing from the best authorities, and deduct 56° for the temperature of the air when the pyrometer stood at 0°. We have 85° degrees of the pyrometer equivalent to 600° of the thermometer, making each degree of the former equal to about 7.0 of the latter.

Having in this manner calculated the value of the degrees, I proceeded to ascertain the equability of the expansion throughout the thermometric scale. The same apparatus was made use of with the addition of a thermometer plunged into the mercury. The heat was applied very carefully and gradually, and the progress of the two instruments was compared at every 50° of the thermometer, both in heating and cooling. The results of the experiment are contained in the following table. The first column contains intervals of 50° of Fahrenheit's thermometer; the second shews what the corresponding degrees of the pyrometer ought to have been, by calculation from the former experiment; the third exhibits the actual ascent of the index while heating; and the fourth its descent when cooling.

Thermometer.	PYROMETER.		
	Calculation.	Experiment.	
		Ascent.	Descent.
50°	7.2°	7.2°	8.0°
100	14.4	14.0	15.5
150	21.6	22.5	23.0
200	28.8	30.5	30.0
250	36.0	38.5	36.5
300	43.2	45.5	43.5
350	50.4	51.5	50.5
400	57.6	58.5	57.5
450	64.8	66.9	65.0
500	72.0	73.5	72.5
550	79.2	77.0	79.7
580	83.6	84.	——
600	86.4	——	——
656	93.6	92	Mercury boils.

The exact point of 600° it was impossible to compare with any safety to a close thermometer. The point of 580° agreed very well in the ascent, but too rapid cooling prevented me from catching it on the return.

When the difficulty of comparing the fine divisions of two instruments so situated, and both in progressive motion, is considered, this will no doubt be regarded as a very close agreement, and quite sufficient to establish the fact of the equability of expansion of both the tube and the metal bar. The experiment has been often repeated with a perfect accordance of result. On one occasion, the lamps employed to heat the mercurial bath had been placed accidentally nearer to one extremity than the other: not having attended to this circumstance, I was surprised not to find the usual accordance between the pyrometer and the thermometer; but, upon considering the matter, I placed a thermometer at each end of the bath, and found that there was a difference of 30° between the two, and the pyrometer indicated as nearly as possible the mean. I was not previously acquainted with the possibility of such a difference existing in a bath of this fluid, however unequally heated; but, upon inquiry since, I find that the remark has been often made. The result has afforded another very satisfactory testimony of the delicacy of the pyrometer, and proves that it will very accurately indicate the mean of the temperature to which the bar is exposed. It is for the purpose of ensuring the application of heat always to the same point exactly, that the shoulder is made on the tube denoting the depth to which the instrument should invariably be immersed.

I shall now proceed to enumerate some precautions which are necessary to be taken in the construction and use of the pyrometer, especially when intended for the observation of very high temperatures, such as that of the fusion of iron. I have selected the black-lead earthen ware, after several trials of other materials, on account of the equability of its expansion, its infusibility at high temperatures, and the perfect manner in which it sustains sudden transitions from heat to cold. I have not only repeatedly taken the tubes at once from a white heat into a cold atmosphere, but have plunged them when red hot into cold water without their sustaining any injury whatever. The heat at which this ware is commonly baked is not very high, and I consider it necessary to ensure accuracy, that the tube should have been exposed for some time to a temperature at least equal to the highest which it is intended to measure with the pyrometer. For this purpose those which I have made use of were placed in an iron-founder's furnace. After this operation they assumed exteriorly a brown appearance, but were as soft and as easily cut as before. When it is required to expose the pyrometer to a naked fire, it is proper to furnish it with an exterior tube of the same, or some other ware which is easily fitted to the shoulder by grinding; otherwise the fuel is apt to act as a flux upon the tube, which, becoming vitrified, will crack with sudden transitions of temperature. This precaution is not so necessary when a muffle is used, but is perhaps always advisable at very high heats.

When it is proposed to keep the pyrometer for a long time in a very strong fire, a piece of cloth may be wrapped round the brass ferule and upper part of the tube, and kept moist with water to guard against too great an accumulation of heat in that part. I have not, however, myself had occasion to make use of this expedient, although I have kept the instrument for half an hour at a time at the temperature of fused iron. The black-lead is so very bad a conductor of heat, that its transmission is very slow from one end to the other.

The value of the degrees of each instrument must be taken for itself, and this is very easily done by means of the apparatus before described. The boiling of mercury furnishes an admirable fixed point for this purpose. The number of degrees equivalent to 656° of Fahrenheit should be marked upon the scale.

The motion of the index, as before stated, is very gradual, and it stops directly the intensity of the fire ceases to increase. The difference of a greater or less draught of air in a furnace is instantly denoted by it. In the furnace which I have been in the habit of employing, the opening of a small door instantly increased the advance of the needle, and the closing it as suddenly checked it. When properly managed in cooling, the needle never failed to return within two or three degrees of the commencement of the scale; but to ensure this effect, it is necessary that the instrument should be cooled very gradually in the furnace to which it has been applied, or that it should be removed suddenly and at once into the cold air. If it be withdrawn gradually, the partial cooling is apt to produce a slight alteration in the form of the tube, the effect of unequal contraction. From this cause I have seen the index not return within twenty or thirty degrees of the point from whence it set out. This small deviation in the form of the tube when it occurs is not of any serious practical consequence as the index may always be set at the beginning of an experiment, to the commencement of the scale, or to the temperature of the air, without in any way affecting the accuracy of the subsequent observations.

I shall conclude by recording the results of some experiments which I have tried upon the fusing point of some of the metals.

I do not offer them as positive and accurate determinations of the different degrees, but only as nearer approximations than any that have yet been furnished from actual observation. The only method which I have yet had it in my power to adopt for this purpose, I do not consider to be susceptible of absolute accuracy. The arrangement made, consisted of a muffle of black-lead placed in an excellent draught-furnace. This muffle was furnished with a door through a round hole in which the stem of the pyrometer was passed up to the shoulder. Another hole in the top of the door which could be stopped at pleasure, admitted a full view of the interior. The metal to be tried was placed in a small black-lead receptacle of the same thickness as the pyrometer tube, in the middle of the muffle. Now it is evident that the pyrometer so situated would indicate the mean heat of the whole of the muffle, which heat might and did vary in different parts. Of two pieces of silver, of the same size, placed within an inch of each other, one fused some time before the other. Every precaution was taken to place the metal to be tried as near as possible to that part where the mean heat probably existed, but still the method is not susceptible of extreme precision. Means might be contrived to surround the instrument with the metal in a state of fusion much in the same manner as it is surrounded with mercury for the purpose of graduation, but this would require particular opportunities which it is to be hoped that those will avail themselves of who have them in their power. The experiments were repeated more than once with a very close agreement of results, but the fusing point of silver is most to be relied on, as having been furnished by three different trials, all of them agreeing to a degree.

		Pyrom.		Therm.
Boiling point of mercury . . .		92	=	644
Fusing point of tin		63	=	441
Ditto	bismuth . . .	66	=	462
Ditto	lead	87	=	609
Ditto	zinc	94	=	648

These four last points were all determined by placing the muffle over two Argand lamps.

	Pyrom.		Therm.
Fusing point of brass	267	=	1869
Ditto pure silver	319	=	2233
Ditto copper	364	=	2548
Ditto gold	370	=	2590
Ditto cast iron	497	=	3479
A red heat just visible in the day-light is about	140	=	980
The heat of a common parlour fire	163	=	1141

The difference between most of these results, and those of Mr. Wedgwood is indeed enormous. His determination of the same points is as follows:—

	Pyrom.		Therm.
Mercury boils	$-3.\frac{673}{1000}$	=	600
Red heat fully visible, in the dark	−1.	=	947
Ditto in the day-light . .	0.	=	1077
Brass melts	+21	=	3807
Copper	27	=	4587
Fine silver	28	=	4717
Fine gold	32	=	5237
Cast iron	130	=	17977

When the nature of the two pyrometers is considered, and the principles upon which they are founded, there will not exist, I trust, much doubt, as to the degree of confidence to which each is entitled. It must be recollected, that the equal expansion of platinum, with equal increments of heat, is one of the best established facts of natural philosophy, while the equal contraction of clay, is an assumption which has been disputed, if not disproved. The instrument which I have proposed, has the further advantage of confirming the indications of its ascent when heating, by its gradual return to its original state when cooling, an advantage which is totally uncompensated in that of Mr. Wedgwood. There is yet, another argument, drawn from a very different method of reasoning, which I think will convince those who are at all conversant with the effects of heat upon metals, and the management of a furnace. Mr. Wedgwood fixes the heat of an enamelling furnace, at 1857°, and the fusing point of fine silver at very considerably more than double, viz., 4717°. Now, any body almost knows, how very soon silver melts after it has attained a bright red heat, and every practical chemist has observed it to his cost, when working with silver crucibles. Neither the consumption of fuel, nor the increase of the air-draught, necessary to produce this effect, can warrant us in supposing that the fusing point of silver is 4½ times higher than a red heat, fully visible in day-light. Neither on the same grounds, is it possible to admit that a full red-heat being 1077°, and the welding heat of iron 12,777°, that the fusing point of cast iron can be more than 5000° higher. The welding of iron must surely be considered as incipient fusion.

Much, however, very much remains to be done in this wide field of research; and when it is considered, what important results have arisen from the accurate estimation of the degrees of heat, comprehended within the scale of that invaluable instrument, the thermometer, it is surely sufficient to inspire ardour in our inquiries, into the almost boundless range of which that instrument measures comparatively so small a part. Happy indeed shall I be, if it shall be found that I have been fortunate enough to suggest the means of advancing one step in a pursuit, which promises so much benefit to science and the arts.

I shall now terminate this paper by the record of two facts, which although not in immediate connexion with the previous inquiry, have arisen collaterally from it, and I believe are new and worthy of attention. The first is, that mercury amalgamates readily with platinum at about its boiling temperature. The combination is very intimate, and it requires a strong red heat to volatilize the mercury from it; the platinum is then left in a honey-combed or dissected state.

The second is, that a piece of cast iron strongly heated, and afterwards slowly cooled in a muffle, became covered with small but very distinct octohëdral and tetrahëdral crystals, of black oxide of iron. The specimen which I have preserved, is very pretty, and the facets of the crystals very perfect and brilliant.

The pyrometers are made and sold by Mr. Newman, Lisle-Street, to whose ready comprehension and execution of my ideas, I am much indebted.

ART. XII. *Reply to some further Observations and Experiments, relative to the Eighth Pair of Nerves, by Dr.* HASTINGS, *Physician to the Worcester Infirmary. By* S. D. BROUGHTON, *Mem. Roy. Col. of Sur., Surgeon to the St. George's and St. James's Dispensary, and in H. M.'s 2d Regt. of Life Guards; in a Letter to the* EDITOR.

DEAR SIR,

In the Observations and Experiments which you did me the favour to publish in January last, I endeavoured to avoid the mazes of speculation and argument, and to confine the attention of my readers to simple deductions from facts. Wishing to avoid every thing likely to promote discussion, my anticipations of setting the question at rest were rendered more confident from the knowledge, that my experiments tended to confirm the opinions of the best physiologists of the day, and probably of a large majority of the profession. Some observations upon my paper, having appeared in your journal for April last, by a Dr. Hastings of Worcester, containing *gross misrepresentations* of my statements, I am somewhat reluctantly compelled to appeal from so very unfair an attack.

When an Author appears purposely striving to misrepresent, some suspicion as to the motive which influences him will naturally arise; and, the impartial reader may probably entertain some doubts respecting the candour of an opponent, when informed that he is the pupil, assistant, and zealous advocate of the doctrines of the master, at whose "*Gamaliel foot*" he has received his education. In fact, Dr. Hastings is the "grand caterer and dry nurse" of Dr. Wilson Philip's experiments, and nervous-galvanic theory. And accordingly while the great man is employed in grappling with "the grown serpent*," it naturally falls to the disciple's charge to deal with the "insolent worm," who presumes to turn upon his Master's doctrine.

The authorities which I quoted were merely intended to exhibit a general view of what is known, relative to the influence of the par vagum in the animal economy, and whether it made for, or against me, was a matter of indifference, as I had no object to serve, but that of submitting truth to the test of experiment, upon a point which seems never till of late, to have been particularly inquired into, though often noticed collaterally. That the result of my observations is not at variance with those quoted, (as Dr. Hastings asserts it to be,) no unbiassed reader can fail to perceive, from the very apparent circumstances of the *variety* of accounts given of the effects of dividing the eighth pair of Nerves, and the express opinion of Le Gallois, that, though the most obviously urgent symptom is often the affection of the stomach, yet he found the disturbance of its functions *extremely variable in degree* in different species of animals, and at different ages, and even in the same species of animals. Dr. Hastings endeavours to shew that Le Gallois does not favour the idea of the disturbance of the stomach arising from that of the lungs. But, it will be found on reference, that he particularly asserts it to be his opinion, that the affection of the stomach is a *secondary* effect, *not constant*, and *variable in degree*, (excepting efforts to vomit, which always occur from the first shock of the operation,) and that that of the thoracic viscera *is constant*, and by its influence on the circulation reduces the functions of life to a state which finally renders them incapable of continuance. So much for my being "contradicted by *all* former and cotemporary experience," and, that "no fact is better made out, than that the gastric secretions immediately cease on dividing the par vagum in the neck universally."

Among our cotemporaries, I am accused of omitting the experiments of a Dr. Clarke Abel, another "Daniel come to judgment." But, though he may possibly be what the *transposition of his names* would unquestionably make him, it was sufficient for my purpose that I stated Dr. Wilson Philip did not stand alone and unsupported in his argument.

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* See the controversy between Dr. Wilson Philip, and Dr. Alison.

Dr. Hastings takes it upon himself to put the ignorant on their guard against being misled by my application of the term *digestion;* and explains to them what it really means. Possibly the reader, like the lady in the farce, may say, "Here are two very civil gentlemen trying to make themselves understood, but the interpreter is the most difficult to comprehend of the two."

Then we have a playful quibble about *mucus* and *chyme,* in which my meaning is ingeniously perverted. I used the word *mucus instead of chyme*, because I wished to state simply the fact as it appeared to me, leaving it open to others to judge from their own experience of the truth of my suggestions*. Had Dr. Hastings followed this general rule, by which I govern myself, in detailing his experiments, it would have been more fair, instead of adopting the *self dictum style*, and thus giving us no means of judging for ourselves. In place of saying such and such were the results of certain experiments, it seems to me more candid to note the *exact appearances,* leaving it open to others to form their own opinions as to the indications of such appearances. For, it is the great misfortune of almost every art and science, that their professors are too apt to imagine theories without the experience of facts, which they afterwards warp to support their own views, rather than in aiding the cause of truth; and which is pretty much the same as erecting a structure before the foundation is secured.

But I have yet a graver charge to bring forward; no less than a palpable misrepresentation, absolutely unwarranted by any ties or debts of gratitude, that may be due between Dr. Wilson

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* Dr. Wilson Philip says, this semi-fluid is the mucus of the stomach, and not chyme. I believe it to be the result of combination of gastric fluid with the parsley, and not secreted originally in the form described.

Philip and his tyro. The reader is informed that in the whole, or the greater part of my experiments no alteration in the food was observable, but in its different degrees of brownness. Whereas, the case really stands thus: not wishing to fatigue my readers with a repetition of the same detail of appearances after every experiment separately and in succession, I contented myself, having once named the appearances after death, with stating that *no deviation in them was observed, in the succeeding experiments, from those noticed antecedently, excepting the brown tint of the parsley;* thereby obviously meaning to imply, that *similar appearances shewed themselves in the succeeding experiments to those of the preceding.* How then can Dr. Hastings venture to state, as a man of veracity, that in a large majority of my experiments, nothing appeared in the food and the stomach when examined, but the brownish colour of the former, and thence argue that the food was unaltered? How can he suppose that I should be incautious enough to bring forward such evidences in proof of the continuance of digestion, if it were as he represents it? I regret that I am compelled to lengthen these remarks, but I must in justice to my own reputation, and to the cause I have espoused, contradict the following assertions:—

First, that I did not compare animals fasted and fed, but not operated upon, with those similarly prepared and operated on. My answer is, that I did so and *have mentioned it.* Secondly, that I did not observe after death whether the eighth pair of nerves in the dog, were divided. I again answer that I did so, and have mentioned it in this, and every other case. As to the objection that this dog may have vomited up his milk at night, since it does not appear that I watched him all night; I certainly did not feel it necessary that I should keep watch, but I took care to place him under such circumstances, as would have rendered it impossible for me not to have perceived it, if he had rejected any more milk after the last he took. Moreover how else came the *whey* to be found in the stomach, and the *curd disappeared,* but from the decomposition of the milk by the gastric fluid, and digestion of the curd?

Thirdly, that the nerves may have re-united. I again answer to this, that I found the divided ends of the nerves uniformly separate and apart from each other. Nor can I credit the possibility of so quick a re-union. From experiments performed by Mr. Sewel at the Veterinary College, I believe it to be impossible, and also that the re-union is slow and imperfect in its sensibility.

Fourthly, that the three horses cited, make against me most fatally. To this last charge I answer that the first died in an hour, so that no inspection was made of the stomach.

The objection to the second horse, which lived *fifty hours, and was quite well twenty-four of them*, is, that the food was not weighed before eaten, and after death. It is true, it was not, because, even Mr. Field's stable-boy could perceive, without the aid of scales, that the quantity of hay found in the stomach was so much less than that eaten, that (as a horse can't vomit,) it was *evident much had been digested, especially as the colon was empty*. As he was eating just before he died, this accounts for the small portion of hay in the stomach. The third horse, was a single instance (of fifteen experiments,) of no digestion having gone on, after division of the nerves; thus shewing the analogy between the experience of former, and cotemporary writers on the subject, and myself, as to the *variability of the effects*, always excepting the advocates of the doctrine which ascribes digestion entirely to the agency of the nerves of the eighth pair.

In the present general eagerness for information and novelty, some of other habits and callings take an interest in speculative inquiries; and among such, many may be seduced into a belief of the most visionary theories. It is right, therefore, that these should be put upon their guard, and assuredly just and natural that I should not allow the force of my experiments and remarks to hazard the danger attendant upon misrepresentations and idle quibbles, for the correction of which, this letter is solely written. Having discharged this duty, I beg leave to decline all further controversy, which might otherwise become as interminable, as it is to all appearance, useless to the cause of science.

I remain, dear Sir, yours, *&c.*
S. D. Broughton.

Great Marlborough-St.,
April 1821.

P.S.—Since the above letter has lain at the Publisher's, I have been gratified by a personal acquaintance with Dr. Wilson Philip; who, with a degree of zeal unrivalled, and *candour* worthy of imitation in *others*, has been occupied of late in retracing the ground formerly trodden; for the purpose of settling the doubts cast upon his alleged results, to the satisfaction (if possible) of all parties. How far this desirable end is attained, the following notice will shew.

After the usual fasting, and subsequent feeding with parsley, the eighth pair of nerves were divided in the necks of three rabbits. One was submitted to the galvanic influence, after the usual manner of Dr. Philip, and the other two were left to themselves. After a few hours the two latter were successively killed, and *in both digestion was perfect*. The galvanised rabbit was then destroyed, and in the stomach of this animal the parsley had undergone *very little alteration*. I may add, with respect to the respiration of the latter rabbit, that, though there was a difference more or less observable at times in its breathing, yet, that it did not continue throughout the experiment to breathe freely and naturally. Whatever difference was observable was *in favour of the galvanic influence*, which was occasionally interrupted for a time.

Dr. Wilson Philip acknowledged (in the most candid manner,) the results of these experiments, so contrary to his alleged constant experience, and felt himself called upon to persevere in his endeavours, to ascertain the cause of such a striking difference between these and Dr. Hastings' results. Several rabbits were subsequently operated upon, in the usual manner, with one exception, and that, it appears, a most important one. The nerves being divided, pains were taken to keep their extremities asunder, so as entirely to prevent any contact between them, a plan always adopted (says Dr. Philip,) by Dr. Hastings, who made *all* Dr. Wilson Philip's experiments for him. In all the rabbits so treated, and some, in which a piece of the nerve was cut out, including two rabbits sent by Dr. Hastings from Worcester after being killed, it was much to my surprise, that though in *all* there was more or less digested parsley to be found, yet in *none* had digestion gone on so completely, as in my own experiments, and in the two first rabbits mentioned above.

The difference indeed was very striking, so much so as to afford strong grounds for supposing that Dr. Wilson Philip was correct in believing that in the different modes of dividing the nerves was to be found the cause of the different results; and there is to be attributed a power in the nerves, to carry on the influence of the brain, after they are divided simply, without any obstruction to their subsequently coming into contact.

At the suggestion of Mr. Andrew Knight, who has taken much interest in these experiments, and afforded us the advantages of his ingenious and philosophic mind, Dr. Wilson Philip once again resorted to his usual experiment with galvanism, having two other rabbits simply operated upon (after the manner of Dr. Hastings,) as a comparative experiment.

The galvanised rabbit had remained singularly quiet the whole time, breathing freely, and with no more apparent distress than the twitches usually produced by the galvanic influence, which in this case was uninterruptedly kept up. The other rabbits laboured strongly in their breathing. They were all three killed at the same period, and their stomachs successively opened. In the two non-galvanised rabbits, digestion had *scarcely made any progress*, but in that galvanised, it was *perfect*, in the manner, to all appearance, avowed by Dr. Wilson Philip and his supporters. However we may differ in opinion, as to the real state of the food in the non-galvanised rabbits, as to Dr. Wilson Philip's theory, or, as to the cause of the formation of chyme and chyle being found under the influence of a galvanic battery, Dr. Wilson Philip cannot be denied the merit of correctness in his assertions, (hitherto almost universally distrusted) relative to the simple fact of a certain power of galvanism producing digestion, after dividing the eighth pair of nerves, under circumstances in which it is impeded without the galvanism.

It is also the merit of Dr. Wilson Philip, that he has discovered cause for believing that nerves can convey the influence of the brain, after being simply divided, and no means used to obstruct their extremities coming into contact.

It is proper to state that the President and several members of the Royal Society, and of the Colleges of Physicians and Surgeons, among whom were Mr. Brodie and myself, inspected the progress of these experiments, which were carried on under the constant superintendence of Dr. Wilson Philip.

I beg leave to take this opportunity of apologising to M. Majendie, who has honoured me by the publication of the greater part of my experiments in his *Journal of Physiology*, for stating that he had not performed similar experiments himself. The error arose from my not, at the time I wrote, having seen the second volume of his *Physiologie*.

It is a great gratification to me to find, that a physiologist of such high reputation as M. Majendie should concur with me on the subject of my experiments; and that his similar results, from dividing the nerves below their distribution to the lungs, favour the idea of the state of the respiration after dividing the nerves above being the probable cause of the interruptions to perfect digestion.

Great Marlborough-Street, June 1821.

ART. XIII. *A Letter to* MR. SAMUEL PARKES, *occasioned by his* Observations *on the* "*Oil Question.*" *By* RICHARD PHILLIPS, F.R.S.E. *&c.*[*]

SIR,

It is not my intention to notice every statement contained in your "*Additional Observations on the Oil Question;* I shall select certain passages to show the character of the means to which you have resorted, for the purpose of obtaining what you term "*triumphant success*," and then leave you to enjoy it.

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[*] See *Journal of Science, Literature, and the Arts*, Nos. XX. and XXI.

For the sake of reference I shall call your first paper the *Observations*, and the second the *Reply;* between the styles of which there is an amusing difference. In the *Observations* you nearly confine yourself to the statement, or misrepresentation or suppression of evidence; but in your *Reply* you take a more elevated flight, and quote Horace and Persius and Virgil, with a facility truly surprising to those who know you.

You must of course be right about *sweet acids* and *silent explosions:* the first expression is correct because you borrowed it; the second is absurd, because it was used by an opponent. *Explosion with noise,* a term which you employ and defend, does not necessarily imply, you say, that there may be *noiseless explosions;* and you inform us in the *Reply* that an *explosion* is "a hissing or an inferior kind of noise;" whilst in the Chemical Catechism, you describe an explosion as having nearly killed Pilatre de Rozier, although it took place only in his mouth; and in your laboratory an *explosion* or inferior hissing noise shattered several twelve-gallon glass receivers into ten thousand pieces. So much for your ridiculous contradictions; I shall presently notice your serious ones.

Speaking of the Associates (meaning those who replied to your *Observations*[*]) you say, (*Reply*, p. 92, note) "they have made two calculations as to the quantity of oil which their vessel would hold, *viz.*, one at p. 44, and the other at p. 51 of their book. And though they are calculations which any schoolboy could have made, they are both erroneous."

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[*] See *Remarks* on a communication published in No. XX. of the *Journal of Science, and the Arts,* entitled 'Observations on the Chemical Part of the Evidence, given upon the late trial of the action brought by Messrs. Severn, King and Co., against the Imperial Insurance Company. By Samuel Parkes, F.L.S., M.G.S., *&c.*' By Richard Phillips, F.R.S.E. *&c.*, Philip Taylor, J.G. Children, F.R.S., *&c.*, John Martineau, Jun^r^, John Bostock, M.D., F.R.S., *&c.*, and John Taylor, M.G.S.

I deny this, and assert that they are both correct. Mr. Children, after mentioning the size of the vessel, *viz.*, 3 feet long, 15 inches wide and 15 inches deep, says " this measurement reduced to cubic inches gives the capacity of the vessel equal to 8100" (*Remarks*, p. 44.) Dr. Bostock having also quoted the dimensions of the vessel as above given, says, " which will give a capacity of 8,100 cubic inches". (*Remarks*, p. 51.) Now unless you are prepared to deny that 15 × 15 = 225 × 36 = 8100, both these " calculations as to the quantity of oil the vessel would hold" are correct.

It is indeed true, that in one part of Dr. Bostock's statement, 6 is erroneously printed instead of 5; but the result is not affected by it, for he has correctly calculated both the capacity of the boiler, and the degree of expansion which the oil undergoes by heating. Mr. Children has certainly committed an error in allowing for the expansion of the oil, but why have you not pointed it out? Because to have made the correction would have convicted you of greater error; and therefore you applied your general rule for unpleasant and inconvenient evidence, and suppressed it. In adding two numbers together, Mr. Children intending to admit even more expansion than you state oil to undergo, allowed the unoccupied space of the boiler to be only 970 instead of 1170 cubic inches.

I next notice the following passage from p. 95 of your *Reply*, " But when I examine the volume which has been put forth, and perceive that the writers have not adduced one new experiment, nor have taken the least pains, *&c. &c.*" Now in your *Observations* (p. 329, note) you say that you are convinced that " no inflammable gas is produced until a portion of the oil becomes actually decomposed, and charcoal formed;" and again, speaking of some gas which you obtained from oil, heated from 590° to 620°, you say, " I have no doubt but the gas, in both instances, was carbonic acid gas; for when cooled, and mixed with atmospheric air, it would not inflame on the application of a match." Once more, in p. 344 of your *Observations*, you say, with respect to " inflammable gas," " I have since the trial, proved by unequivocal experiments, that it cannot be obtained but at a temperature much beyond what our thermometers will measure." Such were your opinions, previously to the printing of our *Remarks*, and let us examine what they contain on the subject. Mr. Philip Taylor, one of the Associates, states, (" volume which has been put forth," p. 30,) that having put a pint of whale oil into a retort, and heated it by a gas light to 620°, he procured an inflammable emanation; and he adds, " after agitating some of this gas with water, I tried its inflammability, and found it very similar to oil-gas. I then received a fresh portion into an air jar, graduated in cubic inches; fifty inches were thus collected, and after standing more than three weeks, in my experiment room, the absorption and condensation only amount to six cubic inches, and the gas continues to exhibit the appearance of oil-gas. There was no formation of charcoal in the retort during the production of the gas."

Now observe the dilemma to which you have reduced yourself: either you knew or you did not know that inflammable gas could be procured in the manner described by Mr. Taylor. If you did not know it then you must have perceived that the Associates have adduced one new experiment; and seeing its importance and its direct bearing on the question, you must have purposely suppressed it. But if, on the other hand, you did know that inflammable gas might be produced from oil without charcoal being formed; that it was not carbonic acid that was evolved at 620°; and if also you knew that inflammable gas might be obtained at a temperature not beyond what our thermometers will measure, then the triumph of which you boast is such as your friends must deplore, and the associates would have spared you.

The next passage which I shall notice occurs in page 98. Here, in consequence of having been charged with garbling and suppressing evidence, you say " can any candid and ingenuous person suppose that I should, without being publicly called upon, have undertaken to give a correct account of the chemical evidence that was adduced on this most important trial, and then have entered upon the task with a determination to conceal some parts, to garble and curtail others, and to misrepresent and pervert what had been adduced by the defendants' witnesses, for the purpose of making a false impression on the public, *&c.* ?' You must be tried by facts and not by professions; and I for one having charged you with concealing, garbling, curtailing and perverting evidence, repeat and will substantiate the charges.

I wish to know whether the public call to which you allude is a mere gratuitous assumption on your part, or rests upon satisfactory evidence: my inquiries have hitherto been unsuccessful, and I am curious to learn when, by whom, or through what channel, this demand was made upon your talents and experience.

With respect to your concealing evidence, several instances were exhibited in the Associates' Remarks; and to one of the most palpable you have not offered even the shadow of a reply; I mean, the charge which I brought against you, of entirely omitting the evidence of Mr. Daniell and Mr. Martineau; because it went to prove that there is no danger in boiling sugar in the common mode, and admitting and enforcing Mr. Robinson's evidence because he thought the boiling dangerous, although he confessed he never knew it set fire to a sugar-house: this I think amounts to concealment. Again, I call your attention to what you have stated in the *Observations*, p. 351, respecting the Juryman's opinion of the oil used in one of our experiments. You were a party to the conversation respecting it, and yet you take advantage of a mistake in the printing of the trial, to insert the word *not*, totally altering what the Juryman said; and not content with this, and effectually to misrepresent what he did say, you have suppressed one half of it; the Juryman is represented in the printed *Trial*, p. 212, as having said, "I think we need not trouble Mr. Taylor at all, we are not satisfied about the oil being pure." You, eager to retain this error of the print, and perceiving that the first part of the sentence was at variance with the second, omitted the words "I think we need not trouble Mr. Taylor at all," and you stated the matter merely thus, "After which one of the Jurymen said, We are not satisfied about the oil being pure." This is I think, an example of your method of curtailing and perverting, and for this you have offered neither defence nor apology.

As to garbling evidence, your treatment of Wilkinson proves this in the clearest manner; he states, and you admit, that the Associates employed a boiler 3 feet long 15 inches wide and 15 deep; and they detail an experiment made with this boiler, in which owing to the formation of a volatile oil, in a fixed one the oil spouted out with considerable force, to the height of several feet. To invalidate this experiment, you have repeatedly stated that the boiler was nearly full, and that the violent expulsion of the oil, which the Associates attributed to the formation of vapour in a viscid fluid, was the effect of common expansion occasioned by heat, on account of the vessel being nearly full and this you assert to have been the case, knowing that Wilkinson had stated that only 24 gallons were put in, which would occupy but little more than two thirds of the boiler, and knowing that his evidence was corroborated by Dr. Bostock, Mr. Taylor, Mr. Faraday and myself. In your *Reply*, how do you attempt to escape from this charge of garbling Wilkinson's evidence? Why, by telling us that he was ignorant of some other matters, and by again garbling his evidence, to convict him of having stated an impossibility. With respect to Wilkinson's evidence generally, I assert that all which he stated *positively* would have been confirmed by Mr. Martineau, had confirmation been required; but to shew that he stated an impossibility, you say, in p. 100 of the *Reply*, that he "had measured 33 gallons of fresh oil into an iron vessel, which, according to his own showing would not hold more than 35 gallons, and had contrived to keep the oil *within this vessel* for five or six days, though it was generally kept at a temperature of 400°; and every one who has made experiments on the expansion of oil, must know that 33 gallons of whale oil measured at the usual temperature of the atmosphere in the month of February, (say from 40° to 50°) would, when brought to the temperature of 400° have measured thirty-nine gallons."

Now in order to convict Wilkinson of the impossibility thus described, you have purposely omitted the word "about" used by him in his evidence, and you have supplied the omission by stating him to have said that he *measured* the oil;—but his words are (*Trial*, p. 147) " I put in about 33 gallons of whale oil." It is evident either that he spoke from memory, or without having measured the oil; and yet you, for the purpose of blasting this man's character, alter his evidence given to the best of his belief, into positive evidence.

In p. 106 of the *Reply*, you say, " Having mentioned the name of Mr. Faraday, it is but justice to that gentleman to state that he had the good sense and the virtue to resist all solicitations to become one of the party," viz., the Associates; I therefore addressed the following letter to Mr. Faraday, and received the annexed answer:

Dear Faraday,

Mr. Parkes, in his " Additional Observations on the Oil Question," has extolled your " good sense and virtue for having resisted all solicitations to become one of the party" who replied to him. I will therefore thank you to inform me, whether, if such solicitations were resisted by you, it arose from any alteration in your opinion as to the danger of heating by means of oil, the nature of the changes which it undergoes by heat, or as to the accuracy of the experiments made by yourself, or conjointly with others?

Yours, very sincerely,

June 3d, 1821. R. PHILLIPS.

Royal Institution, June 5th, 1821.

Dear Phillips,

In answer to your questions I have to state, that I have not been solicited to become one of the party; and that I remember only one occasion on which it was proposed to me to write in answer to Mr. Parkes's observations. I have also to state, that my sole reason for not writing, was disapprobation of controversy, and an opinion that it rarely convinces of error or answers any good purpose; and finally, that my opinion, with regard to the nature of the process, and the changes that take place in the oil, during the heating of it, as well as of the accuracy of the experiments that have been made, and on which that opinion was founded, not only remains unchanged, but is strengthened by my subsequent experience.

I am, dear Sir,
Yours, very truly,
M. FARADAY.

In p. 108 of your *Reply*, alluding to the Directors of the Globe Insurance Company, you assert that " they not only completely abandoned the idea of danger from the oil apparatus," but that they " actually directed the counsel to declare in court, that they were perfectly satisfied, that this apparatus *lessened* the danger."

What truth there is in this declaration, may be learned from the following extract, from the short-hand writer's notes.

Lord Chief Justice Dallas.—" There is one fact, which one way or another would dispose of this cause at once, and save us hours or possibly days of investigation; that is, whether the oil process increased or decreased the danger of fire, or left it as it was before."

Mr. Serjeant Vaughan.—" We propose to prove that it decreases it, my lord."

Mr. Serjeant Bosanquet.—(for the defendants) " I shall certainly submit that is not in the cause." And afterwards,—" It is admitted that the premises were destroyed by an accidental fire, and the defendants agree not to contend that it was by the oil process. We reserve our own opinion, but we do not contend it in this cause."

In p. 112 of your *Reply*, you describe an experiment, in which, after raising oil to 600° you found it impossible to obtain any inflammable product—nay, at 610° the light was extinguished; this is very singular, considering what you have stated in various parts of your remarks. Thus, in your *Observations*, (p. 346,) you assert, in a tone which will have but few imitators, " I know the care with which I made my own experiments, and how the several experiments which were performed by different means corroborated one another; I am therefore satisfied that the results which I obtained were the true results." Now let us examine this agreement of which you boast; in p. 64 of the printed trial, you say that oil heated to 586° gave out inflammable gas, the flame was not permanent, " but at 600° it continued to burn," again, in the *Reply*, (p. 99, note,) you inform us that " oil vapour is not inflammable at the end of a tube, unless the oil be heated to the temperature of about 600° of Fahrenheit." So that according to these careful corroborating experiments, "the true results" are that the vapour of oil at 600° is *not* inflammable, and *is* inflammable, and at 610° extinguishes flame. Although I am heartily tired, I cannot help adding one more example of your agreement in opinion with yourself. Alluding to the forcible expulsion of the oil, described by the Associates, you say (*Observations*, p. 342,) "I cannot conceive how the expansion of the vapour could throw out the oil; for, if the vessel could not hold the vapour and the oil, the natural consequence would have been for the vapour to escape through the tube and not the oil." In the *Reply* (p. 92) we are informed that "this vacuity [in the boiler] would be filled with vapour, in consequence of the large quantity of water which is always formed in oil at a high temperature; and this being generated faster than it could be carried off by the tube, would press with a force on the surface of the oil, that would be sufficient to produce all the effects which they have related;" that is, to drive the oil out of the boiler.

In concluding these remarks, I ask the public to consider what reliance can be placed either upon your own experiments, or upon the representations which you have made respecting the experiments of others; what reliance, I ask with confidence, can be placed upon a man, who, pretending to give an impartial account of the evidence offered during an important trial, suppresses the evidence of two gentlemen out of three who spoke to one particular point, because two out of the three differed from him in opinion; who took advantage of a misprint in the trial, to misrepresent the opinion of a juryman, and even suppresses one half of what that juryman said; who states that two calculations of the capacity of a boiler, are erroneous, which are clearly correct; who says that oil does not give out inflammable vapour without depositing charcoal, and without being heated beyond what our thermometers will measure, and yet, when he is told of these facts, denies that any new experiment is stated; who suppresses repeated evidence as to the quantity of oil which a boiler contained, and in defiance of that evidence states it to have been nearly full; who converts evidence given to the best of belief, into positive evidence, in order to make it contradictory; who states Mr. Faraday to have resisted solicitations which he never heard of; who asserts that the Globe Insurance Company declared their opinion that the oil process lessened the danger, when their counsel declared that they reserved their opinion on the subject; who tells us that oil vapour at 600° is and is not inflammable; and lastly, who asserts that if a vessel contain oil and vapour, and the vapour be generated so fast that the vessel cannot contain both, that the vapour would be expelled and not the oil, but, who afterwards says that oil would be expelled and not the vapour.

Determining not to notice any future remarks of yours, on this subject, I am, yours, *&c.*,

R. PHILLIPS.

ART. XIV. *Proceedings of the Royal Society.*

The following papers, have been read at the table of the Royal Society, since our last report.

April 5. On the mean density of the earth, by CHARLES HUTTON, L.L.D.

On the separation of iron from other metals, by J. F. W. HERSCHEL, Esq.

12. On the restoration of a portion of the urethra in the perinæum, by H. EARLE, Esq., communicated by the President.

May 3. Observations on the variation of local heat made amongst the Garrow Hills, by D. SCOTT, Esq., in a letter to W. T. BRANDE, Esq., Sec., R S.

On some subterraneous trees discovered at the foot of the cliffs, about a mile to the eastward of Mundsley, by Lieut. JEFFERSON MILES, R.N., in a letter to W. T. BRANDE, Esq., Sec., R.S.

Case of a diseased enlargement of the glands of the neck, by JOHN HOWSHIP, Esq., communicated by Sir EVERARD HOME, Bart., V.P.R.S.

May 10. Some remarks on Meteorology, by LUKE HOWARD, Esq.

A Calculation of some observations of the Solar Eclipse, on the 7th of September, 1820, by Mr. CHARLES RUMKER, communicated by Dr. THOMAS YOUNG, Sec. R. S. for For. Corres.

24. On the Anatomy of certain parts of the globe of the eye, by ARTHUR JACOB, M.D., communicated by Dr. JAMES MACARTNEY.

ART. XV.—ANALYSIS OF SCIENTIFIC BOOKS.

I. *A Dictionary of Chemistry, on the Basis of Mr. Nicholson's, in which the Principles of the Science are investigated anew, and its Applications to the Phenomena of Nature, Medicine, Mineralogy, Agriculture, and Manufactures, detailed. By* ANDREW URE, M.D., Professor of the Andersonian Institution, *&c. &c. With an Introductory Dissertation, containing instructions for converting the alphabetical Arrangement into a systematic Order of Study.*—London, 1821.

IT appears to us, that the misconception of a single word, has frequently led chemical teachers and writers into strange errors of arrangement. The term *Element* in reference to philology, arithmetic, geometry, and music, denotes not only whatever is most elementary in principle, but whatever is simplest in conception, in these sciences. From this two-fold concurrence, we are warranted in employing the letters of the alphabet, numeral notation, general axioms, and the diatonic scale, as the primary objects of contemplation to their students. But *chemical elements* appertain to a far different order of conceptions. Instead of being the objects which naturally present themselves at first to the inquirer, they form, for the most part, the ultimate points of his researches. Their simplicity is but a name relative to the stage of our advancement in the science. The bodies which we at present call chemical elements, are probably all compounds; and are certainly the least easy of apprehension to the learner, because they possess properties the most remote from those of the bodies with which his senses are daily conversant, and are disentangled from combination by processes not a little circuitous and intricate.

On the other hand, as the common and obvious properties of matter constitute the elementary principles of general physics, these justly form the initiatory propositions. This condition of natural philosophy allows the plan of tuition to resemble an architectural process, in which a house of a symmetrical plan is regularly raised from its foundations. But the actual state of chemistry is not susceptible of the same comparison. It may be likened more properly to a tree, whose trunk and main branches are easily traced and delineated, but the efflorescence which terminates the boughs, requires the most delicate microscopic research before we can discover the vital germ, the true element of fructification. Hence we perceive that elementary instruction in chemistry, is not that which treats, first of the elementary or undecompounded bodies; but, that which beginning with the familiar and tangible objects of study, like the trunk and branches in the preceding comparison, progressively advances to the more recondite. For this reason, we think that one of the earliest subjects to which the teacher ought to direct a pupil's mind, is *water ;* first, in its various forms as modified by temperature ; and next, as to purity and composition. The disengagement of its two elements from their liquid repose, having familiarized the mind to gaseous existences, he will then readily enter on the chemical examination of the *atmosphere*, and of the curious bodies resulting from the union of its two constituents in successive multiple proportions. An acquaintance with gasometry, and with Gay-Lussac's beautiful doctrine of volumes, both essential to the ulterior investigations, will thus be insensibly formed.

The next transition in this *natural* order of tuition, may be to the well known combustibles, *sulphur* and *charcoal,* and to their several compounds with the bodies previously examined. *Common salt* ought now to be carefully studied, particularly in reference to its constituent *chlorine.* The ores of a few leading metals, with the principles of their reduction, and the metallic oxides, chlorides, and alloys, may now come under review; after which the alkalis, earths, and their saline combinations, with the acids previously examined, viz., the sulphuric, nitric, and carbonic, will be natural objects of research.

By such a course of experimental discipline, the diligent student will acquire, almost imperceptibly, or at least without any perplexity of intellect, a precise conception of the principal objects and methods of chemical research. He may now proceed to examine the various acids and their bases, and the combinations of one or other of these, with the metallic oxides and metals. The salts, and the native mineral and organic productions, will conclude his course. In proportion as he proceeds, the general facts of combination and decomposition, or the laws of chemical affinity, will be developed and fixed in his mind.

Such may be reckoned the best mode of procedure in a course of private study ; but transferred to a book, it would evidently produce apparent disorder, though in reality far more conducive to the instruction of the *regular* reader, than those works which place the elementary bodies at the beginning, and the more familiar bodies, or native compounds, towards the end. This dilemma, between what best promotes the symmetry of a printed treatise, and the edification of its reader, is, with regard to arrangement, nearly unavoidable in the present state of chemistry. The perception of this difficulty has led chemists, at different times, to resort, with much advantage, to the dictionary form, or alphabetical order, for describing the objects and phenomena of their science. Macquer's Dictionary was long a popular work all over Europe, highly creditable to its celebrated author, and was several times republished in the English language. Nicholson constructed, in 1795, on that model, his quarto dictionary, a work distinguished for perspicuity of style, and parallel views of the lately co-reigning chemical hypotheses, the phlogistic and antiphlogistic. In 1808, after having conducted his Philosophical Journal with candour, diligence, and urbanity, he published his octavo dictionary of chemistry, which, though compiled with less diligence and discernment, than his high character as a journalist gave reason to expect, was well received by the public. It presented, at a moderate price, and in a condensed form of typography, a great accumulation of chemical details. Nothing, however, shews more remarkably the slovenliness with which several of the articles were got up, than a comparison of them with the corresponding articles in Messrs. Aikin's quarto dictionary, published the preceding year. In almost every thing belonging to mineral analysis, and particularly to that of the ores, Mr. Nicholson was content to transcribe, or rather to reprint from his former dictionary, the obsolete and defective processes of Cramer, instead of drawing his analytical methods from the more recent and valuable researches of Vauquelin, Klaproth, and Hatchett. Hence, though Nicholson's octavo dictionary, from its price and form, had an extensive sale among chemical students and manufacturers, it never possessed much authority with men of science.

After an interval of twelve years from its publication, in which eventful period, discoveries of greater splendour, variety, and importance, had been added to the science, than during a century before, the proprietor of the copyright of the book took it into his head to print a new edition, and requested Dr. Ure, as we learn from the preface, to superintend its revision for the press. It would appear, that the Doctor had contracted the serious obligation of editorship, for a very trifling sum, without duly considering the great difficulty of revising and printing, within six months, a multifarious work, which required to be, for the greater part, re-written; and that, after the agreement had been made, he was left to struggle through the irksomeness of his task, with no hope of recompense, except the credit of its execution, or the consciousness of deserving well of the chemical world. " The dissertations," says Dr. Ure, " on *Caloric*, *Combustion*, *Dew*, *Distillation*, *Electricity*, *Gas*, *Light*, *Thermometer*, &c., which form a large proportion of the volume, are beyond the letter and spirit of my engagement with the publisher. I receive no remuneration for them, not even at the most moderate rate of literary labour; they are, therefore, voluntary contributions to the chemical student, and have been substituted for what I deemed frivolous and uninteresting details, on some unimportant dye-stuffs, and articles from old dispensatories, such as althea, chamomile, *&c.*" After this statement, he would be an unreasonable critic who should censure Dr. Ure for having re-printed from the old work several indifferently-written articles, since those which he has himself composed, as denoted by asterisks, would constitute nearly three volumes of the ordinary octavo form.

The memoirs which have been published in the *Philosophical Transactions*, and in this journal, by Dr. Ure, would satisfy the world, that, during his long career in public teaching, he has not been a passive spectator of chemical events, but that he has devoted a large proportion of his time to experimental research, without which discipline, indeed, the demonstrations of a Professor, however showy, will possess neither unity of design, nor authoritative force. He would become merely the retailer of another's wares, of whose value, genuineness, and mode of production, he was incompetent to judge. The dictionary affords abundant evidence that its author does not belong to this class of teachers.

The promise held out in the title-page seems to be honestly fulfilled; for he has, on several important occasions, *investigated anew* the principles of chemical science; and has, in consequence, rectified many errors which had gained currency in our compilations. His general views of chemical theory appear to be, for the most part, much sounder than those which have been re-echoed, with unvarying monotony, in what have been called chemical systems. He has, in particular, bestowed much pains in arranging the valuable facts belonging to the English school of chemistry, which, originating with the faultless memoirs of Cavendish, Hatchett, and Howard, on the Baconian plan of research, has finally, under Dalton, Wollaston, and Sir H. Davy, risen to undisputed pre-eminence among the schools of Europe. The numerous facts inconsistent with the Lavoisierian creed are carefully detailed, and the just inferences drawn from them, both with regard to the theory of acidification and combustion.

The general article Acid, in the dictionary, presents, in the first place, Lavoisier's notion of the origin of acidity, with Berthollet's judicious remarks on it. Dr. Ure then exhibits Sir H. Davy's more just and comprehensive views of acid constitution; and concludes with Dr. Murray's hypothetical modification of these views, which he successfully controverts.

> "The more recent investigations of chemists on fluoric, hydriodic, and hydrocyanic acids have brought powerful analogies in support of the chloridic theory, by shewing that hydrogen alone can convert certain undecompounded bases into acids well characterized, without the aid of oxygen. Dr. Murray indeed has endeavoured to revive and new-model the early opinion of Sir H. Davy, concerning the necessity of the presence of water, or its elements, to the constitution of acids. He conceives that many acids are ternary compounds of a radical with oxygen and hydrogen; but that the two latter ingredients do not necessarily exist in them in the state

of water. Oil of vitriol, for instance, in this view, instead of consisting of 81.5 real acid, and 18.5 water in 100 parts, may be regarded as a compound of 32.6 sulphur + 65.2 oxygen + 2.2 hydrogen. When it is saturated with an alkaline base, and exposed to heat, the hydrogen unites to its equivalent quantity of oxygen, to form water which evaporates, and the remaining oxygen and the sulphur combine with the base. But when the acid is made to act on a metal, the oxygen partly unites to it, and hydrogen alone escapes."

"Carbonic acid he (Dr. Murray) admits to be destitute of hydrogen; yet its saturating power is very conspicuous in neutralizing dry lime. Now, oxalic acid, by the last analysis of Berzelius, contains no hydrogen. It differs from the carbonic only in the proportion of its two constituents. And oxalic acid is appealed to by Dr. Murray as a proof of the superior acidity bestowed by hydrogen.

"On what grounds he decides carbonic to be a feebler acid than oxalic, it is difficult to see. By Berthollet's test of acidity, the former is more energetic than the latter, in the proportion of 100 to about 58; for these numbers are inversely as the quantity of each requisite to saturate a given base. If he be inclined to reject this rule, and appeal to the decomposition of the carbonates by oxalic acid, as a criterion of relative acid power, let us adduce his own commentary on the statical affinities of Berthollet, where he ascribes such changes not to a superior attraction in the decomposing substance, but to the elastic tendency of that which is evolved. Ammonia separates magnesia from its muriatic solution at common temperatures; at the boiling heat of water, magnesia separates ammonia. Carbonate of ammonia, at temperatures under 230°, precipitates carbonate of lime from the muriate; at higher temperatures, the inverse decomposition takes place with the same ingredients. If the oxalic be a more energetic acid than the carbonic, or rank higher in the scale of acidity, then, on adding to a given weight of liquid muriate of lime, a mixture of oxalate and carbonate of ammonia, each in equivalent quantity to the calcareous salt, oxalate of lime ought alone to be separated. It will be found, on the contrary, by the test of acetic acid, that as much carbonate of lime will precipitate as is sufficient to unsettle these speculations."

Under Acid (Arsenious) we find a very copious account of its poisonous operation on the living body, as also of its tests and antidotes.

"We may here remark, however, that the most elegant mode of using all these precipitation reagents is upon a plane of glass, a mode practised by Dr. Wollaston in general chemical research, to an extent, and with a success, which would be incredible in other hands than his. Concentrate by heat in a capsule the suspected poisonous solution, having previously filtered it if necessary. Indeed, if it be very much disguised with animal or vegetable matters, it is better first of all to evaporate to dryness, and by a few drops of nitric acid to dissipate the organic products. The clear liquid being now placed in the middle of the bit of glass, lines are to be drawn out from it in different directions. To one of these a particle of weak ammoniacal water being applied, the weak nitrate of silver may then be brushed over it with a hair pencil. By placing the glass in different lights, either over white paper or obliquely before the eye, the slightest change of tint will be perceived. The ammoniaco-acetate should be applied to another filament of the drop, deut-acetate of iron to a third, weak ammoniaco-acetate of cobalt to a fourth, sulphuretted water to a fifth, lime-water to a sixth, a drop of violet syrup to a seventh, and the two galvanic wires at the opposite edges of the whole. Thus with one single drop of solution many exact experiments may be made."

A plane of white paper answers extremely well for the same microscopic tests, as recommended by Dr. Paris.

The proportional or atomic weight of alum, or prime equivalent, as Dr. Ure calls it, he deduces from Sir H. Davy's experiments, to be 32, and not 21.36, as assigned by Berzelius, for Dr. Wollaston's scale. Our author seems successful in reconciling the constitution of alum with the former number.

> **" It deserves to be remarked (says he), that the analysis of Professor Berzelius agrees with the supposition that alum contains**

4	**sulphuric acid**	**=**	**20.0**	**34.36**
2	**alumina**	**=**	**6.4**	**11.00**
1	**potash**	**=**	**6.0**	**10.30**
23	**water**	**=**	**25.8**	**44.34**
			58.2	**100.00**

> **" If we rectify Vauquelin's erroneous estimate of the sulphate of barytes, his analysis will also coincide with the above. Alum, therefore, differs from the simple sulphate of alumina previously described, which consisted of three prime equivalents of acid, and two of earth, merely by its assumption of a prime of sulphate of potash."**

Under Aluminite, he shews that this mineral

> **" May be represented very exactly by**

2	**primes of acid**	**10.**	**=**	**20.**
5	**——— alumina**	**16.**	**=**	**32.**
21	**——— water**	**23.6**	**=**	**47.2**
	Foreign matter	**0.4**	**=**	**0.8**
		50.0		**100.0**

> **" The conversion of the above into alum is easily explained. When the three primes composing bisulphate of potash come into play, they displace precisely three primes (or atoms) of alumina. Two additional primes of water are also introduced at the same time, by the strong affinity of the bisulphate for the particles of that liquid."**

Subjoined to the article Attraction (Chemical) we were glad to see Dr. Young's ingenious tables of affinity, which have been unaccountably omitted in our large systematic works.

Caloric is a very elaborate article, and, considering the circumstances under which the dictionary was re-written, it is one which will be read with interest. His division of the subject seems to be clear and comprehensive.

> **" Enough has now been said to shew how little room there is to pronounce dogmatic decisions on the abstract nature of heat. If the essence of the cause be still involved in mystery, many of its properties and effects have been ascertained, and skilfully applied to the cultivation of science and the uses of life.**
>
> **" We shall consider them in the following order:**
>
> **" 1. Of the measure of temperature.**
>
> **" 2. Of the distribution of heat.**
>
> **" 3. Of the general habitudes of heat with the different forms of matter."**

The *distribution* of heat he subdivides into two parts: 1st., the mode of distribution, or the laws of cooling, and the communication of heat, among aëriform, liquid, and solid substances; and, 2dly, the specific heats of different bodies, at the same and at different temperatures. In treating of the third head, the general habitudes of heat with the different forms of matter, he says,

> **" The effects of heat are either transient or physical; or permanent and chemical, inducing a durable change in the constitution of bodies. The second mode of operation we shall treat of under Combustion. The first falls to be discussed here; and divides itself naturally into the two heads, of changes in the volume of bodies while they retain their form, and changes in the state of bodies."**

Several newly constructed and very valuable tables will be found interspersed through this article. The following practical applications of the doctrines previously inculcated, will afford an example of our author's powers of popular description:

"But the most splendid trophy erected to the science of caloric, is the steam-engine of Watt. This illustrious philosopher, from a mistake of his friend Dr. Robison, has been hitherto defrauded of a part of his claims to the admiration and gratitude of mankind. The fundamental researches on the constitution of steam, which formed the solid basis of his gigantic superstructure, though they coincided perfectly with Dr. Black's results, were not drawn from them. In some conversations with which this great ornament and benefactor of his country honoured me, a short period before his death, he described, with delightful *naïveté*, the simple, but decisive, experiments by which he discovered the latent heat of steam. His means and his leisure not then permitting an extensive and complex apparatus, he used apothecaries' phials. With these, he ascertained the two main facts, first, that a cubic inch of water would form about a cubic foot of ordinary steam, or 1728 inches; and that the condensation of that quantity of steam would heat six cubic inches of water from the atmospheric temperature to the boiling point. Hence he saw that six times the difference of temperature, or fully 900° of heat, had been employed in giving elasticity to steam; which must be all abstracted before a complete vacuum could be procured under the piston of the steam-engine. These practical determinations he afterwards found to agree pretty nearly with the observations of Dr. Black. Though Mr. Watt was then known to the Doctor, he was not on those terms of intimacy with him, which he afterwards came to be, nor was he a member of his class.

"Mr. Watt's three capital improvements, which seem to have nearly exhausted the resources of science and art, were the following:—1. The separate condensing chest, immersed in a body of cold water, and connected merely by a slender pipe with the great cylinder, in which the impelling piston moved. On opening a valve or stop-cock of communication, the elastic steam which had floated the ponderous piston, rushed into the distant chest with magical velocity, leaving an almost perfect vacuum in the cylinder, into which the piston was forced by atmospheric pressure. What had appeared impossible to all previous engineers was thus accomplished. A vacuum was formed without cooling the cylinder itself. Thus it remained boiling hot, ready the next instant to receive and maintain the elastic steam. 2. His second grand improvement consisted in closing the cylinder at top, making the piston-rod slide through a stuffing box in the lid, and causing the steam to give the impulsive pressure, instead of the atmosphere. Henceforth the waste of heat was greatly diminished. 3. The final improvement was the double impulse, whereby the power of his engines, which was before so great, was in a moment more than doubled. The counter-weight required in the single-stroke engine, to depress the pump-end of the working beam, was now laid aside. He thus freed the machine from a dead weight or drag of many hundred pounds, which had hung upon it from its birth, about seventy years before.

"The application of steam to heat apartments, is another valuable fruit of these studies. Safety, cleanliness, and comfort, thus combine in giving a genial warmth for every purpose of private accommodation, or public manufacture. It has been ascertained, that *one cubic foot* of a boiler will heat about *two thousand feet* of space, in a cotton-mill, whose average heat is from 70° to 80° Fahr. And if we allow 25 cubic feet of a boiler for a horse's power in a steam-engine supplied by it, such a boiler would be adequate to the warming of fifty thousand cubic feet of space. It has been also ascertained that one square foot of surface of steam-pipe is adequate to the warming of two hundred cubic feet of space. This quantity is adapted to a well-finished ordinary brick or stone building. The safety valve on the boiler should be loaded with two pounds and a half for an area of a square inch, as is the rule for Mr. Watt's engines. Cast-iron pipes are preferable to all others, for the diffusion of heat. Freedom of expansion must be allowed, which in cast-iron may be taken at about a tenth of an inch for every ten feet in length. The pipes should be distributed within a few inches of the floor.

> "Steam is now used extensively for drying muslin and calicoes. Large cylinders are filled with it, which, diffusing in the apartment a temperature of 100° or 130°, rapidly dry the suspended cloth. Occasionally the cloth is made to glide in a serpentine manner closely round a series of steam cylinders, arranged in parallel rows. It is thus safely and thoroughly dried in the course of a minute. Experience has shewn, that bright-dyed yarns like scarlet, dried in a common *stove* heat of 128°, have their colour darkened, and acquire a harsh feel; while similar hanks, laid on a *steam*-pipe heated up to 165°, retain the shade and lustre they possessed in the wetted state. The people who work in steam drying-rooms are healthy; those who were formerly employed in the stove-heated apartments, became soon sickly and emaciated. These injurious effects must be ascribed to the action of cast iron, at a high temperature, on the atmosphere."

The true theory of the formation of coal-gas is clearly stated in the following short paragraph.

> "If coal be put into a cold retort, and slowly exposed to heat, its bitumen is merely volatilized in the state of condensible tar. Little gas, and that of inferior illuminating power, is produced. This distillatory temperature may be estimated at about 600° or 700° F. If the retort be previously brought to a bright cherry-red heat, then the coals, the instant after their introduction, yield a copious supply of good gas, and a moderate quantity of tarry and ammoniacal vapour. But when the retort is heated to nearly a white incandescence, the part of the gas richest in light, is attenuated into one of inferior quality, as I have shewn in detailing Berthollet's experiments on CARBURETTED HYDROGEN. A pound of good cannel coal, properly treated in a small apparatus, will yield five cubic feet of gas, equivalent in illuminating power to a mould candle six in the pound. See CANDLE."

We meet with a number of useful rules for conducting chemical computations, in different parts of the dictionary. The reader will find several curious ones in the last article, as well as under GAS. The article *Combustion* may be regarded as presenting Dr. Ure's ideas of chemical theory. We shall quote one or two passages from it.

> "COMBUSTION. The disengagement of heat and light which accompanies chemical combination. It is frequently made to be synonymous with inflammation, a term which might be restricted, however, to that peculiar species of combustion in which gaseous matter is burned. Ignition is the incandescence of a body, produced by extrinsic means, without change of its chemical constitution.
>
> "Beccher and Stahl, feeling daily the necessity of fire to human existence, and astonished with the *metamorphoses* which this power seemed to cause charcoal, sulphur, and metals to undergo, came to regard combustion as the single phenomenon of chemistry. Under this impression, Stahl framed his chemical system, the *Theoria Chemiæ Dogmaticæ*, a title characteristic of the dogmatic spirit with which it was inculcated by chemical professors, as the infallible code of their science for almost a century. When the discoveries of Scheele, Cavendish, and Priestley, had fully demonstrated the essential part which air played, in many instances of combustion, the French school made a small modification of the German hypothesis. Instead of supposing, with Stahl, that the heat and light were occasioned by the *emission* of a *common* inflammable principle from the combustible itself, Lavoisier and his associates dexterously availed themselves of Black's hypothesis of latent heat, and maintained, that the heat and light emanated from the oxygenous air, at the moment of its union or fixation with the inflammable basis. How thoroughly the chemical mind has been perverted by these conjectural notions, all our existing systems of chemistry, with *one* exception, abundantly prove.
>
> "Dr. Robison, in his preface to Black's lectures, after tracing, with perhaps superfluous zeal, the expanded ideas of Lavoisier to the neglected germs of Hooke and Mayhow, says, 'This doctrine concerning combustion, the great, the characteristic phenomenon of chemical nature, has at last

received almost universal adoption, though not till after considerable hesitation and opposition; and it has made a complete revolution in chemical science.' The French theory of chemistry, as it was called, or hypothesis of combustion as it should have been named, was for some time classed in certainty with the theory of gravitation. Alas! it has vanished with the luminous phantoms of the day; but the sound logic, the pure candour, the numerical precision of inference, which characterize Lavoisier's elements, will cause his name to be held in everlasting admiration.

" It was the rival logic of Sir H. Davy, aided by his unrivalled felicity of investigation, which first recalled chemistry from the pleasing labyrinths of fancy, to the more arduous but far more profitable and progressive career of reason. His researches on combustion and flame, already rich in blessings to mankind, would alone place him in the first rank of scientific genius. I shall give a pretty copious account of them, since by some fatality it has happened, that in our best and largest system, where so many pages are devoted to the reveries of ancient chemists, the splendid and useful truths made known by the great chemist of England have been totally overlooked.

" Whenever the chemical forces which determine either combination or decomposition are energetically exercised, the phenomena of combustion, or incandescence with a change of properties, are displayed. The distinction, therefore, between supporters of combustion and combustibles, on which some late systems are arranged, is frivolous and partial. In fact, one substance frequently acts in both capacities, being a supporter *apparently* at one time, and a *combustible* at another. But in both cases the heat and light depend on the same cause, and merely indicate the energy and rapidity with which reciprocal attractions are exerted.

" Thus, sulphuretted hydrogen is a combustible with oxygen and chlorine; a supporter with potassium. Sulphur, with chlorine and oxygen, has been called a combustible basis; with metals it acts the part of a supporter; for incandescence and reciprocal saturation result. In like manner, potassium unites so powerfully with arsenic and tellurium as to produce the phenomena of combustion. Nor can we ascribe the phenomena to extrusion of latent heat, in consequence of condensation of volume. The protoxide of chlorine, a body destitute of any combustible constituent, at the instant of decomposition, evolves light and heat with explosive violence; and its volume becomes one-fifth greater. Chloride and iodide of azote, compounds alike destitute of any inflammable matter, according to the ordinary creed, are resolved into their respective elements with tremendous force of inflammation; and the first expands into more than 600 times its bulk. Now, by the prevailing hypothesis of latent heat, instead of heat and light, a prodigious *cold* ought to accompany such an expansion. The chlorates and nitrates, in like manner, treated with charcoal, sulphur, phosphorus, or metals, deflagrate or detonate, while the volume of the combining substances is greatly enlarged. The same thing may be said of the nitrogurets of gold and silver. In truth, the combustion of gunpowder, a phenomenon too familiar to mankind, should have been a bar to the reception of Lavoisier's hypothesis of combustion. The subterfuges which have been adopted, and admitted, in order to reconcile them, are unworthy to be detailed.

" From the preceding facts, it is evident, 1st, That combustion is not necessarily dependent on the agency of oxygen; 2d, That the evolution of the heat, is not to be ascribed simply to a gas parting with its latent store of that ethereal fluid, on its fixation, or combustion; and, 3dly, That ' no peculiar substance or form of matter is necessary for producing the effect, but that it is a *general* result of the actions of any substances possessed of strong chemical attractions, or different electrical relations, and that it takes place in all cases in which an intense and violent motion, can be conceived to be communicated to the corpuscles of bodies.'

" All chemical phenomena indeed may be justly ascribed to motions among the ultimate particles of matter, tending to change the constitution of the mass.

" It was fashionable for awhile, to attribute the caloric evolved in combustion, to a diminished capacity for heat of the resulting substance. Some phenomena, inaccurately observed, gave rise to this generalization. On this subject I shall content myself with stating the conclusions to which MM. Dulong and Petit have come, in consequence of their own recent researches on the laws of heat, and those of Berard and Delaroche. ‘ We may likewise,’ say these able chemists, ‘ deduce from our researches another very important consequence for the general theory of chemical action; that the quantity of heat developed at the instant of the combination of bodies, has no relation to the capacity of the elements; and that in the greatest number of cases, this loss of heat is not followed by any diminution in the capacity of the compounds formed. Thus, for example, the combination of oxygen and hydrogen, or of sulphur and lead, which produces so great a quantity of heat, occasions no greater alteration in the capacity of water, or of sulphuret of lead, than the combination of oxygen with copper, lead, silver, or of sulphur with carbon, produces in the capacities of the oxides of these metals, or of carburet of sulphur.’—‘ We conceive, that the relations which we have pointed out between the specific heats of simple bodies, and of those of their compounds, prevent the possibility of supposing, that the heat developed in chemical actions, owes its origin merely to the heat produced by change of state, or to that supposed to be combined with the material molecules.’—*Annales de Chimie et Physique*, x.

“ Mr. Dalton, in treating of the constitution of elastic fluids, lays it down as an axiom, that diminution of volume, is the criterion of chemical affinity being exercised; and hence maintains, that the atmospheric air is a mere mixture. Thus, also, the extrication of heat from chemical union, has been usually referred to the condensation of volume. The following examples will shew the fallacy of such crude hypotheses. 1. Chlorine and hydrogen mixed, explode by the sunbeam, electric spark, or inflamed taper, with the disengagement of much heat and light; and the volume of the mixture, which is greatly *enlarged* at the instant of combination, suffers no condensation afterwards. Muriatic acid gas, having the mean density of its components, is produced. 2. When one volume of olefiant gas and one of oxygen are detonated together, three and a half gaseous volumes result, the greater part of the hydrogen remains untouched, and a volume and a half of carbonic oxide is formed, with about 1-10th of carbonic acid. 3. The following experiments of M. Gay-Lussac on liquid combinations are to the same purpose. 1. A saturated solution of nitrate of ammonia, at the temperature of 61°, and of the density 1.302, was mixed with water in the proportion of 44.05 to 33.76. The temperature of the mixture sank 8.9°; but the density at 61° was 1.159, while the mean density was only 1.151. 2. On adding water to the preceding mixture, in the proportion of 33.64 to 39.28, the temperature sank 3.4°, while the density continued 0.003 above the mean. Other saline solutions presented the same result, though none to so great a degree.

“ That the internal motions which accompany the change in the *mode* of combination, independent of change of *form*, occasion the evolution of heat and light, is evident from the following observations of Berzelius:—In the year 1811, when he was occupied with examining the combinations of antimony, he discovered, accidentally, that several metalline antimoniates, when they begin to grow red-hot, exhibit a sudden appearance of fire, and then the temperature again sinks to that of the surrounding combustibles. He made numerous experiments to elucidate the nature of this appearance, and ascertained that the weight of the salt was not altered, and that the appearance took place without the presence of oxygen. Before the appearance of fire, these salts are very easily decomposed, but afterwards they are attacked neither by acids nor alkaline leys—a proof that their constituents are now held together by a stronger affinity, or that they are more intimately combined. Since that time he has observed these appearances in many other bodies, as, for example, in green oxide of chromium, the oxides of tantalum and rhodium.—See CHROMIUM.

“ Mr. Edmund Davy found, that when a neutral solution of platinum was precipitated by hydro-sulphuret of potash, and the precipitate dried in air deprived of oxygen, a black compound was obtained, which when heated out of the contact of air, gave out sulphur, and some sulphuretted hydrogen gas, while a combustion similar to that in the formation of the metallic sulphurets appeared, and common sulphuret of platinum remained behind. When we heat the oxide of rhodium, obtained from the soda-muriate, water first comes over; and on increasing the temperature, combustion takes place, oxygen gas is suddenly disengaged, and a suboxide of rhodium remains behind. The two last cases are analogous to that of the protoxide of chlorine, the *euchlorine* of Sir H. Davy. Gadolinite, the siliciate of yttria, was first observed by Dr. Wollaston to display a similar lively incandescence. The variety of this mineral, with a glassy fracture, answers better than the splintery variety. It is to be heated before the blowpipe, so that the whole piece becomes equally hot. At a red-heat it catches fire. The colour becomes greenish-grey, and the solubility in acids is destroyed. Two small pieces of gadolinite, one of which had been heated to redness, were put in aqua regia; the first was dissolved in a few hours; the second was not attacked in two months. Finally, Sir H. Davy observed a similar phenomenon on heating hydrate of zirconia.

"The *verbal* hypothesis of thermoxygen by Brugnatelli, with Dr. Thomson's supporters, partial supporters, and semicombustion, need not detain us a moment from the substantial facts, the noble truths, first revealed by Sir H. Davy, concerning the mysterions process of combustion. Of the researches which brought them to light it has been said, without any hyperbole, that 'if Bacon were to revisit the earth, this is exactly such a case as we should choose to place before him, in order to give him, in a small compass, an idea of the advancement which philosophy has made since the time, when he had pointed out to her the route which she ought to pursue.'

"The coal mines of England, alike essential to the comfort of her population and her financial resources, had become infested with fire-damp, or inflammable air, to such a degree as to render the mutilation and destruction of the miners, by frequent and tremendous explosions, subjects of sympathy and dismay to the whole nation. By a late explosion in one of the Newcastle collieries, no less than one hundred and one persons perished in an instant; and the misery heaped on their forlorn families, consisting of more than three hundred persons, is inconceivable. To subdue this gigantic power was the task which Sir H. Davy assigned to himself; and which, had his genius been baffled, the kingdom could scarcely hope to see achieved by another. But the stubborn forces of nature can only be conquered, as Lord Bacon justly pointed out, by examining them in the nascent state, and subjecting them to experimental interrogation, under every diversity of circumstance and form. It was this investigation which first laid open the hitherto unseen and inaccessible sanctuary of *Fire*."

We recommend the whole article to the diligent perusal of our readers. He has transplanted with fidelity the beautiful and invaluable facts, first disclosed to the world in Sir H. Davy's papers on flame, published in the *Philosophical Transactions*. Dr. Ure arranges the phenomena of combustion under six heads:—

"1*st*, The temperature necessary to inflame different bodies. 2*d*, The nature of flame, and the relation between the light and heat which compose it. 3*d*, The heat disengaged by different combustibles in burning. 4*th*, The causes which modify and extinguish combustion, and of the safe-lamp. 5*th*, Invisible combustion. 6*th*, Practical inferences.

Our author has announced in the introduction, a systematic work on chemistry.

"If the public," says he, "after this larger specimen of my chemical studies, shall deem me qualified for the task, I may promise its completion within a year from this date. The work will be comprised in four octavo volumes, and will contain the results of numerous investigations into the various objects of practical chemistry, joined to a systematic view of its principles. By several simple instruments, tables, and rules of calculation, chemical analysis, the highest and most intricate part of the science, may, I apprehend, be, in many cases, brought within the reach of the busy manufacturer; while, by the same means, such accuracy and despatch may be ensured, as to render the analysis of saline mixtures, complex minerals, and mineral waters, the work of an hour or two; the proportions of the constituents being determined, to one part in the thousand."

The success, which the present work has already had, will, we hope, encourage Dr. Ure to bring forth, with all diligence, a work so much wanted at the present moment, as that above-described. From the specimens he has exhibited in the Dictionary, we are satisfied that it will contain a faithful exposition of the known facts, with useful rules for simplifying chemical practice.

The Dictionary would have been improved, for occasional consultation, by page numerals, for want of which, there is a difficulty of referring to specified passages, in such extensive dissertations, as his articles ATTRACTION, CALORIC, COMBUSTION, ELECTRICITY, GAS, EQUIVALENTS CHEMICAL, *&c.* The view of what is usually called the atomic theory, which he has given under the last-named article, seems to us the most complete and most philosophical hitherto offered to the public, and presents some valuable rules for computing from analysis, the proportional weight, or prime equivalent of the various simple and compound bodies. O.

ii. *The Elements of Chemical Science. By* J. GORHAM, M.D., *Member of the American Academy, and Professor of Chemistry in Harvard University, Cambridge.* 2 *vols.* 8*vo. Boston,* 1819 *and* 1820.

This is the first original book on Chemistry published in the United States, and merely as such, deserves the notice of our chemical readers; it contains nothing original either in experiment or observation, neither does it profess originality, but it has the merit of perspicuous arrangement and candid narrative.

The first volume includes a succinct account of the doctrines of attraction, heat, light, and electricity, of the chemical properties of the supporters of combustion, and of the simple inflammable unmetallic bases, and concludes with a general view of the process of combustion, and of the analogies between the simple substances and some of their compounds, with remarks on some points of chemical theory. The preliminary part of this volume is a judicious compilation of facts from the best English works; but, in the concluding chapters, Dr. Gorham enters upon general views, and becomes more tangible to the critic. In his history of combustion, the author has scarcely done justice to Jean Rey who preceded Hooke and Mayow, and whose experiments tended to establish the influence of air in that process, and to lead to views analogous to those subsequently adopted by his eminent successors; nor has he given as much room to the experiments and speculations of Hooke as they deserve, while he has detailed, with tiresome minuteness, the exploded theory of Lavoisier, and has even condescended to notice and transcribe, not, it is true, without some animadversion, the Thomsonian theory of semi-combustion, upon which we have formerly expressed our opinion. In this part of his book Dr. Gorham has also wasted some pages in detailing the absurd speculations respecting the absolute zero, placed by Dr. Irvine at 700°., and by Mr. Dalton at 6150°. below 0, this small discrepance being of little importance, it would seem, in such inquiries. There is also much that might have been omitted in relation to the capacities of bodies for heat; but we can forgive these exuberances, as they lead to a tolerable compendium of Sir H. Davy's researches on flame, and to an exposition of those inimitable investigations which ended in his discovery of the safety lamp.

In Dr. Gorham's chapter on some points of chemical theory, he adverts to the imperfections of all systematic arrangements of the subjects of Chemistry, and to the difficulties which beset the writer in concisely setting forth his materials without frequent repetition, or more blamable omission of facts small in themselves but important in association. "If," says he, "we commence with the principles of the science, its laws can be demonstrated only by a reference to the mutual actions of bodies still unknown; and if individual substances be first described, the general terms which have been appropriated to classes of facts must be employed, and frequently but imperfectly explained." Of these difficulties, and of many others, those who are at all conversant with chemical writers, must be amply aware, and, in the present state of the science, we see little probability of their removal, or even of their material diminution. Taking all things into the account, we are of opinion that Dr. Gorham has himself followed one of the least exceptionable plans of arrangement; namely, that which, after having discussed the general laws of chemical changes, proceeds to the history of elementary bodies, and of their mutual combinations in inorganic nature, and, ultimately, to their complex arrangements in the animal and vegetable world. We are quite aware that this plan is not very philosophical, but upon such a subject we willingly sacrifice logical accuracy to perspicuous detail, and consider that as the best arrangement, which most easily enables the student to retain, compare, and apply the infinitely numerous, diversified, and scattered facts of this endless branch of natural knowledge.

In adverting to the analogies between the elementary substances, Dr. Gorham has struck out nothing new, and has not been peculiarly happy in retailing the opinions of others; chemical writers indeed generally misemploy their own and their readers' time, in entering upon the abstract philosophy of their science, and would fill their paper more advantageously in extending practical details, and describing the minutiæ of manipulation. Could we see a System of Chemistry from the pen of Dr. Wollaston, or of Sir Humphry Davy, we should indeed expect to be instructed and edified by their incursions into the truly philosophical and speculative regions of their science; the former would surprise us by the profound and accurate solidity of his judgment in regard to theoretical points; and the latter would enchain our attention by the brilliancy of his generalizations, and the happy talent which he so eminently possesses, of seizing upon remote analogies and bringing them to bear upon new investigations and discoveries. But as we fear that neither Dr. Wollaston nor Sir H. Davy, who already sit "enthroned in the uppermost chambers" will ever condescend to the drudgery of compilation which has not inaptly been compared to the labour of the anvil and the forge, we must rest content with, and should indeed feel grateful to, those who employ that measure of time and talent which they possess, in arranging and collecting the insulated and scattered facts of chemistry into a tangible aggregate, provided they perform their task with judgment and candour, and not in that sour and distorting vein of peevish petulance for which we have lately had occasion to reprimand a writer, whose general information and indefatigable diligence promised at one time to render him eminent amongst British Systematists. But, to be brief, we would have chemical writers bestow more time upon the real business of the laboratory, and less upon matters of opinion and speculation; not in detailing the figures of retorts and receivers, nor in descanting upon the art of filling soap bubbles with hydrogen gas, but in describing faithfully and minutely the various obstacles that oppose the student's progress in the common processes of experiment, as well as in the more refined and difficult branches of analysis. We are no admirers of Scientific Catechisms, nor do we profess profound reverence for Messrs. Longmans' manufactory of Philosophical Conversations; but we venture to suggest that even from these humble sources, for it is said there are sermons even in stones, some hints might be derived, useful to the systematic writer; at least, he might learn from them the mode of addressing beginners in the study, which though he is too apt to forget it, constitutes a main part of his calling as an instructor.

But, to return to Dr. Gorham; the second volume includes the history of the metals and of organic substances. As our author has omitted all mineralogical and geological details, he might have greatly improved his introductory chapter on the metals, if he had sketched their natural history; he might also have introduced in this place, some few historical particulars respecting them, which are interesting and important to the student, and which we have looked for in vain, in other parts of his book; with these exceptions, however, Dr. G. has given a good general sketch of the chemical habitudes of this class of bodies.

In considering them individually he has adopted the arrangement of Thenard, which, with all its faults, is perhaps, as little open to objection as any other which could be suggested; excepting that we should deem it improved by placing the division which treats of bodies regarded analogically as metallic oxides, last instead of first.

To give our readers some notion of Dr. Gorham's individual treatment of the metals, we shall select one of the most important, namely, Iron; the general order of description is similar in all of them. The first paragraphs relate to the importance of this metal, to its general diffusion, and its mechanical properties; its two oxides are next described, and the difficulty adverted to of reconciling their composition with the atomic theory. The chlorides, carburets, phosphuret, and sulphurets of iron are then treated of, and to these succeeds the history of its salts, in the order following: 1. Chlorate. 2. Muriate. 3. Nitrate. 4. Carbonate. 5. Phosphate. 6. Sulphate. 7. Ferrocyanate. The last paragraph of the section on iron describes its alloys. Such is the general arrangement adopted under the head of each of the metals, and it is in our opinion infinitely preferable to that disjointed plan, generally followed by our own authors, of considering the metals in the abstract in one chapter, their salts in a second, their combinations with oxygen in a third, and so forth; the perplexity and confusion of which, if not self-evident, may be amply judged of by reference to Dr. Thomson's system, and to M. Thenard's *Traité*. While however we applaud our author's plan, we cannot congratulate him on the happiness of its execution; his details are meagre and unsatisfactory; we are told nothing of the ores of the metals; of the means of reducing them; of the methods of analyzing their combinations; of their uses in the arts; and of many other things which Dr. Gorham might have picked out of the works of Hatchett, Klaproth, and other standard authorities, and which would greatly have contributed to the value of his work, more especially considered as a text book for students. The speculations of Dr. Berzelius, and the hypotheses of Dr. Murray, are very well as speculations and hypotheses, but they should not have been suffered to usurp the place of genuine philosophy.

The epitome of Vegetable Chemistry is divided into three chapters; the first is subdivided into twenty sections, giving an account of the proximate principles of plants; the second chapter contains a brief view of the structure and chemical physiology of vegetables, which would have more aptly preceded the former; and the third is entitled, "Of the Spontaneous Decomposition of Vegetables," and includes the phenomena and products of fermentation.

The fluids and solids of the animal body, and the changes that attend their spontaneous decomposition, are the subjects of the two concluding chapters; to the latter, "*mineral waters*" are tacked on as a kind of outrider; how these are connected with, or related to toasted cheese and adipocere, we cannot even guess; the printer, probably, is to blame; Dr. Gorham, however, gives a most faulty sketch of the analysis to be adopted in ascertaining their constituents; and what is less pardonable, he refers to threadbare and insufficient authorities for further details; Phillips, Marcet, and Klaproth, are the sources to which he should have directed his readers.

Having already said that Dr. Gorham's book contains nothing either new or original, we have not thought it necessary to canvass the theories which he has adopted; of the arrangement we have already spoken in terms of sufficient approbation; but, having also briefly adverted to some of its defects, we doubt not that in a future edition we shall see considerable improvement. Unfortunately every page of this work shows that the author is not at home in the laboratory; although therefore his reading is extensive, and has made him well acquainted with the labours of others, there is a want of that free and easy description which we meet with in the writings of really practical chemists, and which makes the reader, as it were, an assistant and participator in the processes that are brought before him. Dr. Gorham's style and language, though without elegance, are sufficiently correct and unobjectionable, but for the reasons we have just stated, his book is dry and uninteresting to any except the mere tyro; he has every where done ample justice to the British school of Chemistry, and in the introduction to the first volume, has given a very creditable sketch of the causes which have influenced the recent progress of the science, and which have tended to annihilate the visionary and speculative generalizations of Lavoisier and his associates.

I.

ART. XVI. ASTRONOMICAL AND NAUTICAL COLLECTIONS. No. VI.

i. *A Postscript on Atmospherical Refraction. By* THOMAS YOUNG, M.D., F.R.S. *From the* Philosophical Transactions *for* 1819. *With a parenthetical Correction.*

1. A SIMPLE and convenient method of calculating the precise magnitude of the atmospherical refraction, in the neighbourhood of the horizon, has generally been considered as almost unattainable; and Dr. Brinkley has even been disposed to assert the "impossibility of investigating an exact formula," [that should represent all its variations], notwithstanding the "striking specimens of mathematical skill, which," as he justly observes, "have been exhibited in the inquiry." We shall find, however, that the principal difficulties may be evaded, if not overcome, by some very easy expedients.

2. The distance from the centre of the earth being represented by x, and the weight of the superincumbent column by y, the actual density may be called z, and the element of y will vary as the element of x and as the density conjointly; consequently, $\mathrm{d}y = -mz\mathrm{d}x$; the constant quantity m being the reciprocal of the modulus of elasticity. The refractive density may be called $1 + pz$, p being a very small fraction; and it is easy to see that the perpendicular u, falling on the direction of the light, will always vary inversely as the refractive density, since that perpendicular continually represents the sines of the consecutive angles, belonging to each of the concentric surfaces at which the refraction may be supposed to take place (Nat. Phil. II. p. 81:) and $u = \frac{s}{1+pz}$, s being a constant quantity. The angular refraction at each point will obviously be directly as the elementary change of this perpendicular, and inversely as the distance v from the point of incidence; whence the fluxion of the refraction will be $\frac{\mathrm{d}u}{v} = \mathrm{d}r$, as is already well known.

3. For the fluent of this expression, which cannot be directly

integrated, we may obtain a converging series by means of the Taylorian theorem; but we must make the fluxion of the refraction constant, and that of the density variable; so that the equation will be $u = \frac{d\mathrm{u}}{dr} . r + \frac{d^2\mathrm{u}}{dr^2} . \frac{r^2}{2} + \frac{d^3\mathrm{u}}{dr^3} . \frac{r^3}{2.3} + \ldots$, u being the initial value of u, when $r = 0$. Now the whole variation, of which u is capable, while z decreases from 1 to 0, extends from $\frac{s}{1+p}$ to s; or, since p is very small, from $s - ps$ to s; and dr being $= \frac{du}{v}$, we have the equation $ps = vr + \frac{dv}{dr} . \frac{r^2}{2} + \ldots$

But $v = \sqrt{}\,(x^2 - u^2)$, $dv = \frac{xdx - udu}{v}$, and $\frac{dv}{dr} = \frac{x}{v} . \frac{dx}{dr} - u$; and dx being $= -\frac{dy}{mz}$, and $du = -psdz$, $\frac{dx}{dr} = \frac{v}{mpsz} . \frac{dy}{dz}$.

4. We must now determine the value of the density z, which, when the temperature is uniform, becomes simply $= y$; but for which we must find some other function of y, including the variation of temperature; and we may adopt, for this purpose, the hypothesis lately advanced by Professor Leslie, in the article Climate of the Encyclopædia Britannica, and suppose the density to be augmented, by the effect of cold, in the proportion of 1 to $1 + n\left(\frac{1}{z} - z\right)$, n being somewhat less than $\frac{1}{10}$; and since the density is as the pressure and the comparative specific gravity conjointly, we have $z = y\left(1 + n\left[\frac{1}{z} - z\right]\right)$, $\frac{z}{y} = 1 + \frac{n}{z} - nz$, $d\frac{z}{y} = \frac{dz}{y} - \frac{zdy}{yy} = -\frac{ndz}{zz} - ndz$, and $\frac{dy}{dz} = \frac{y}{z} + \frac{nyy}{z^3} + \frac{nyy}{z}$; consequently $\frac{dx}{dr} = \frac{v}{mpsz}\left(\frac{y}{z} + \frac{nyy}{z^3} + \frac{nny}{z}\right)$, $\left(\frac{dy}{ydr} = -\frac{mz}{y} . \frac{dx}{dr} = -\frac{v}{psz} - \frac{nvu}{psz^3} - \frac{nvy}{psz}\right.$, and not "$\frac{-v}{psz} - \frac{2nvy}{psz^3} - \frac{2nvy}{psz}$," which stands in the original paper, in manifest defiance of *the first rules of arithmetic:* indeed the *very first* rule of all is forgotten, for the paragraphs are *numbered* 1, 2, 3, 4, 6 . . . ! But

instead of retracing the steps of the calculation with these corrections only, it will be more satisfactory to extend the general theorem somewhat further, without confining it to a particular law of temperature.

5. For this purpose we may make $\frac{dy}{dz} = \zeta$, $\frac{d\zeta}{dr} = \zeta'$, $\frac{d\zeta'}{dr} = \zeta''$, and we shall have, for computing the coefficients of the series $vr + \frac{dv}{dr} \cdot \frac{rr}{2} + \ldots$, the values

$$dr = \frac{du}{v};$$

$$\frac{du}{dr} = v;$$

$$\frac{dx}{dr} = \frac{\zeta v}{mpsz};$$

$$\frac{dz}{dr} = \frac{-du}{psdr} = \frac{-v}{ps};$$

$$\frac{dy}{dr} = \frac{\zeta dz}{dr} = \frac{-\zeta v}{ps};$$

$$\frac{dv}{dr} = \frac{\zeta x}{mpsz} - u;$$

$d\,\frac{dv}{dr} = d\zeta . \frac{x}{mpsz} + dx . \frac{\zeta}{mpsz} - dz . \frac{\zeta x}{mpsz} - dz \frac{\zeta x}{mpsz^2} - v dr$; and $\frac{ddv}{dr^2} = \frac{\zeta' x}{mpsz} + \frac{\zeta^2 v}{m^2 p^2 s^2 z^2} + \frac{\zeta x v}{mp^2 s^2 z^2} - v$. Now since m is about 766, it is obvious that the second term, containing its square, may be neglected in comparison with the third, since the other quantities concerned in these terms can never differ materially from each other: for the same reason the term v may be omitted, as not being divided by p, and u may be considered as equal to s, and its fluxion neglected, as well as that of x, which may be called $= 1$; and we may proceed to take the fluxion of $\frac{ddv}{dr^2} = \frac{\zeta' x}{mpsz} + \frac{\zeta x v}{mp^2 s^2 z^2} = \frac{\zeta'}{mpsz} + \frac{\zeta v}{mp^2 s^2 z^2}$; whence $d\,\frac{ddv}{dr^2} = \frac{d\zeta'}{mpsz} - \frac{\zeta' dz}{mpsz^2} + \frac{\zeta dv + v d\zeta}{mp^2 s^2 z^2} - \frac{2\zeta v dz}{mp^2 s^2 z^3}$; and $\frac{d^3 v}{dr^3} = \frac{\zeta''}{mpsz} + \frac{\zeta' v}{mp^2 s^2 z^2} + \frac{\zeta}{m^2 p^2 s^2 z^2} . \left(\frac{\zeta}{mpsz} - s\right) + \frac{\zeta' v}{mp^2 s^2 z^2} + \frac{2\zeta v^2}{mp^3 s^3 z^3}$. It will now be convenient to divide ζ'' into the two portions $\zeta''_{,}$ and $\zeta''_{,,}\, v^2$, in order to obtain that part of the sixth term which is independent of v; the fourth will then become $\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{\zeta''_{,}}{mpsz} + \frac{\zeta}{mp^2s^2z^2} \cdot \left(\frac{\zeta}{mpsz} - s\right) + \left(\frac{\zeta''_{,,}}{mpsz} + \frac{2\zeta'}{vmp^2s^2z^2} + \frac{2\zeta}{mp^3s^3z^3}\right) v^2$. The whole of the fluxion of the former part will contain v, which will disappear again in the next term, being changed into $\mathrm{d}v$, and the v^2 of the second part will become $2\frac{\mathrm{d}v^2}{\mathrm{d}r^2}$ in the sixth term. We shall therefore have, for the case of the horizontal refraction, when $z = 1$ and $s = 1$, $\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = \left(\frac{\mathrm{d}\zeta''_{,}}{mp\mathrm{d}r} - \frac{\zeta''_{,}}{mp} \cdot \frac{\mathrm{d}z}{\mathrm{d}r} + \frac{\zeta'}{mp^2} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} - \frac{2\zeta\mathrm{d}z}{mp^2\mathrm{d}r} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta}{mp^2} \cdot \frac{\mathrm{dd}v}{\mathrm{d}r^2}\right) \frac{\mathrm{d}v}{v\mathrm{d}r} + 2\frac{\mathrm{d}v^2}{\mathrm{d}r^2}\left(\frac{\zeta''_{,,}}{mp} + \frac{2\zeta'}{vmp^2} + \frac{2\zeta}{mp^3}\right)$.

It is obvious, that since $\frac{\mathrm{dd}v}{\mathrm{d}r^2} = \left(\frac{\zeta'}{v} \cdot \frac{1}{mpsz} + \frac{\zeta}{mp^2s^2z}\right) v$, the quantity $\zeta''_{,}$ must be derived from it by taking the fluxion with respect to v only, and must be equal to $\frac{\mathrm{dd}v}{\mathrm{d}r^2} \cdot \frac{\mathrm{d}v}{v\mathrm{d}r}$, which is the product of the second and third coefficients. The fluxion of this quantity, $d\zeta''_{,}$ is also capable of a simpler expression; for since ζ' will in general be divisible by v, $\zeta''_{,} = \frac{\mathrm{d}'\zeta'}{\mathrm{d}v} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} = \frac{\zeta'}{v} \cdot \frac{\mathrm{d}v}{\mathrm{d}r}$, and $\mathrm{d}\frac{\zeta'}{v} = \frac{\mathrm{d}\zeta'}{v} - \frac{\zeta'\mathrm{d}v}{vv}$; whence $\frac{\mathrm{d}\zeta''_{,}}{\mathrm{d}r} = \frac{\mathrm{d}\zeta'}{v\mathrm{d}r} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} - \frac{\zeta'}{vv} \cdot \frac{\mathrm{d}v^2}{\mathrm{d}r^2} + \frac{\zeta'}{v} \cdot \frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \frac{\zeta''}{v} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} - \frac{\zeta''\mathrm{d}v}{v\,\mathrm{d}r} + \frac{\zeta'}{v} \cdot \frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \frac{\zeta''_{,,}v^2}{v} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta'}{v} \cdot \frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \zeta''_{,}\, v \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta'}{v}\frac{\mathrm{d}^2v}{\mathrm{d}r^2}$. Consequently

$$\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = \left(\frac{\zeta''_{,,}}{mp} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta'}{v^2mp} \cdot \frac{\mathrm{d}^2v}{\mathrm{d}r^2} + \frac{\zeta'}{vmp^2} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta'}{vmp^2} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{2\zeta}{mp^3} \cdot \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\zeta}{vmp^2} \cdot \frac{\mathrm{dd}v}{\mathrm{d}r^2}\right) \frac{\mathrm{d}v}{\mathrm{d}r} + \frac{\mathrm{d}v^2}{\mathrm{d}r^2}\left(\frac{2\zeta''_{,,}}{mp} + \frac{4\zeta'}{vmp^2} + \frac{4\zeta}{mp^3}\right)$$

$$= \frac{\mathrm{d}v^2}{\mathrm{d}r^2}\left(\frac{3\zeta''}{mp} + \frac{6\zeta'}{vmp^2} + \frac{6\zeta}{mp^3}\right) + \frac{\mathrm{d}v}{\mathrm{d}r}\left(\frac{\zeta'}{v} + \frac{\zeta}{p}\right)\frac{\mathrm{dd}v}{vmp\mathrm{d}r^2}$$

6. We may next proceed to substitute, in these general ex-

pressions, the values derived from the various laws which may be supposed to govern the variations of temperature: observing first, that in general

$m = 766, p = .000\,2825 = \frac{1}{3540}$; whence

$\frac{1}{mp} = 4.621,\ \frac{1}{m^2p^2} = 21.3536,\ \frac{1}{mp^2} = 16358,\ \frac{1}{m^2p^3} = 72052,$

$\frac{1}{mp^3} = 57\,907\,320.$

7. (A) If the temperature were uniform, we should have $y = z, \mathrm{d}y = \mathrm{d}z, \zeta = 1, \zeta' = 0, \zeta'' = 0$, and $\frac{\mathrm{d}v}{\mathrm{d}r} = \frac{1}{mps} - s = \frac{4.621}{s} - s$; and when $s = 1$, 3.621.

$$\frac{\mathrm{d}^2v}{\mathrm{d}r^2} = \frac{v}{mp^2s^2}$$

$$\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{1}{mp^2s^2}\left(\frac{1}{mps} - s\right) + \frac{2v^2}{mp^3s^3};\ \text{or if } v = 0,\ 16358 \times 3.621.$$

$$\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = \left(\frac{1}{mp} - 1\right)^2 \cdot \frac{6}{mp^3} + \left(\frac{1}{mp} - 1\right) \cdot \frac{1}{m^2p^4} = 3.621\ (6 \times 57907320$$

$\times\ 3.621 + 16358^2) = 5524050000$; $\frac{1}{720}$ of which is 7672300. Hence, for $s = 1$, we have the equation $.0002825 = 1.8105\, r^2 + 2467\, r^4 + 7672300\, r^6 + \ldots$, in which, if we put $r^2 = .000130$, we shall have $.0002825 = .0002939 + \ldots$; which is too much: then taking $r^2 = .000120$, we have $.0002825 = .00021726 + .00003552 + .00001325 + [.00001647]$: and this is somewhat too great a remainder; for the quotients of the terms being 6, 3..., the remainder ought not to exceed the last term; so that r^2 must be about .000121, and $r = .0110$, or 37′ 50″, which is too great by about one ninth By the assistance of this series we might easily compute the refraction upon the hypothesis of Professor Bessel, who supposes the variation of density to follow the same law as if the temperature were uniform, but alters the value of *m*, so as to accommodate it to the actual magnitude of the refraction in low altitudes.

(B) In Professr Leslie's hypothesis, we have

$n = \frac{45}{500} = .09$

$$\zeta = \frac{y}{z} + \frac{nyy}{z} + \frac{nyy}{z^3};\ \text{the initial value } \zeta^{\smile} = 1 + 2n = 1.18$$

$$\zeta' = \frac{v}{ps}\left(\frac{y}{z^2} + \frac{nyy}{z^2} + \frac{3nyy}{z^4}\right) - \frac{v\zeta}{ps}\left(\frac{1}{z} + \frac{2ny}{z} + \frac{2ny}{z^3}\right)$$

$$\zeta'^{\smile}\frac{v}{ps}(1+4n)(1-\zeta) = \frac{-v}{ps}(2n + 8n^2)$$

$$\frac{d^2v^{\smile}}{dr^2} = \frac{1 + 2n - (2n + 8n^2)}{mp^2s^2}\, v = \frac{1-8n^2}{mp^2s^2}\, v.$$

$$\zeta'' = \frac{d'\zeta'}{dv}\cdot\frac{dv}{dr} + \frac{d'\zeta'}{dy}\cdot\frac{dy}{dr} + \frac{d'\zeta'}{dz}\cdot\frac{dz}{dr} + \frac{d'\zeta'}{d\zeta}\cdot\frac{d\zeta}{dr} = \zeta''_{,} + \zeta''_{,}\, v^2$$

$$\zeta''_{,} = \frac{d'\zeta'}{dv}\cdot\frac{dv}{dr} = \frac{\zeta'}{v}\cdot\frac{dv}{dr} = \frac{\zeta'}{v}\left(\frac{1+2n}{mpsz} - s\right)$$

$$\zeta''_{,,}\, v^2 = \frac{d'\zeta'}{dv}\cdot\frac{-\zeta v}{ps} + \frac{d'\zeta'}{dz}\cdot\frac{-v}{ps} + \frac{d'\zeta'}{d\zeta}\cdot\zeta'$$

$$\zeta''_{,,}\, v^{2\smile} = \left\{\frac{v}{ps}(1+8n) - \frac{\zeta v}{ps}(4n)\right\}\cdot\frac{-\zeta v}{ps} - \frac{v}{ps}\left\{\frac{-v}{ps}(2 + 14n) + \frac{v\zeta}{ps}\right.$$
$$\left.(1+8n)\right\} + \frac{v^2}{p^2s^2}(1+4n)(2n+8n^2) = \frac{v^2}{p^2s^2}\cdot\Big(-\zeta(1+8n) + \zeta^2(4n)$$
$$+2+14n - \zeta(1+8n) + 2n + 16n^2 + 32n^3\Big)$$
$$= \frac{v^2}{p^2s^2}(-1-8n-2n-16n^2+4n+16n^2+16n^3+2+14n-1-8n$$
$$-2n-16n^2+2n+16n^2+32n^3)$$
$$= \frac{v^2}{p^2s^2}\cdot(48n^3)$$

$$\frac{d^3v^{\smile}}{dr^3} = \frac{1}{mp^2s^2}\left\{-(2n+8n^2)\left(\frac{1+2n}{mps} - s\right) + \zeta\left(\frac{1+2n}{mps} - s\right)\right\}$$
$$+ \frac{v^2}{mp^3s^3}(48n^3 - (4n+16n^2) + 2 + 4n)$$
$$= \frac{1-8n^2}{mp^2s^2}\left(\frac{1+2n}{mps} - s\right) + \frac{2+16n^2+48n^3}{mp^3s^3}\, v^{\smile}.$$

$$\frac{{}^5dv^{\smile}}{dr^5} = \left(\frac{1+2n}{mp} - 1\right)^2\cdot\frac{1}{mp^3}(144n^3 - 12n - 48n^2 + 6 + 12n) + \left(\frac{1+2n}{mp}\right.$$
$$\left.-1\right)\cdot\frac{1-8n^2}{m^2p^3}\left(\frac{1+2n}{p} - \frac{2n+8n^2}{p}\right) = \left(\frac{1+2n}{mp} - 1\right)^2\cdot\frac{1}{mp^3}$$
$$(6 - 48n^2 + 144n^3) + \left(\frac{1+2n}{mp} - 1\right)\left(\frac{1-8n^2}{mp^2}\right)^2.$$

We have then, for the case of horizontal refraction, $\frac{\mathrm{d}v}{\mathrm{d}r} = 4.453 = 2 \times 2.2265$; $\frac{\mathrm{d}^3v}{\mathrm{d}r^3} = \frac{.9352}{mp^2} \times 4.453 = 68112 = 24 \times 2838$, and $\frac{\mathrm{d}^5v}{\mathrm{d}r^5} = (4.453)^2 \times 57907320 \times 5.7162 + 4.453 \times 15296^2 = 7657200\,000 = 720 \times 10635000$: consequently, $.0002825 = 2.2265\, r^2 + 2838\, r^4 + 10635000\, r^6$; now if $r^2 = .0001$, we have $.0002825 = .00022265 + .00002838 + .000010635$ [$+ .000020935$]: consequently, .0001 is too little for r^2, and we may try .00011, giving $.0002825 = .00024491 + .00003434 + .00001420$ [$- .00001095$]. But in order to keep up the probable sequence of the progression, the remainder should be about equal to the last term, or about .000011, and .0000209 should have been diminished by about .00001 instead of .0000318; so that we must take .000103 as the true value of r^2 on this hypothesis, and $r = 34' 53''$, which is *exactly*] too great by about $1'$; a difference by far too considerable to be attributed to the errors of observation only; and we must infer, that the law of temperature, obtained from the height of the line of congelation, is not correctly true, if applied to elevations remote from the earth's surface. [If indeed this law were fully established, and capable of being applied, with any little modification, to the exact computation of the refraction, it would be necessary, for the lowest altitudes, either to compute a greater number of the fluxional coefficients, or to divide the refraction into two or more parts, and determine the successive changes of density required for each of them. We should also have] for finding, on this hypothesis, the height x, corresponding to the pressure y and the density z, the expression $mx - m = 1 - \frac{y}{z} + \frac{n}{q}\ \mathrm{hl}\ \frac{2zz + gy\,(1 - z)}{2zz - qy\,(1 - z)}$; y being $= \frac{zz}{z + n - nzz}$, and $q^2 = 1 + 4n^2$[: and the actual state of the atmosphere would probably be very well represented by this formula, taking $n = .1$ or $.11$, rather than .09.

(C)]Professor Bessel's hypothesis is also found to make the horizontal refraction too great. Mr. Laplace's formula, which affords a very correct determination of the refraction, is said to agree sufficiently well with direct observation also; but, in fact, this formula gives a depression considerably greater than was observed by Gay Lussac, in the only case which is adduced in its support; and the progressive depression follows a law which appears to be opposite to that of nature, the temperature varying less rapidly at greater than at smaller heights, while the observations of Humboldt and others seem to prove that in nature they vary more rapidly. Notwithstanding, therefore, the ingenuity, and even utility of Mr. Laplace's formula, it can only be considered as an optical hypothesis, and we are equally at liberty to employ any other hypothesis which represents the results with equal accuracy; or even to correct our formulas by comparison with astronomical observations only, without assigning the precise law of temperature implied by them.

[D. We may compute the effect of a temperature supposed to vary uniformly with the height, by making $z = y\ (1 + tx - t)$, or $= yx^t$, we have then $\frac{z}{y} = 1 + tx - t$, or x^t, and $\mathrm{d}\frac{z}{y} = \frac{\mathrm{d}z}{y} - \frac{z\mathrm{d}y}{yy}$ $= t\mathrm{d}z$, or $= tx^{t-1}\mathrm{d}x$, which are initially the same. But $t\mathrm{d}x = \frac{-t\mathrm{d}y}{mz}$, and $\frac{\mathrm{d}z}{y} = \frac{z\mathrm{d}y}{yy} - \frac{t\mathrm{d}y}{mz}$, whence $\frac{\mathrm{d}z}{\mathrm{d}y} = \frac{z}{y} - \frac{ty}{mz} = \frac{mzz - tyy}{myz}$, and $\frac{\mathrm{d}y}{\mathrm{d}z} = \zeta = \frac{myz}{mzz - tyy}$; consequently $\mathrm{d}\zeta = \frac{my\mathrm{d}z + mz\mathrm{d}y}{mzz - tyy} . - 2myz . \frac{mz\mathrm{d}z - ty\mathrm{d}y}{(mzz - tyy)^2}$; and initially $\zeta = \frac{m}{m - t}$, and $\zeta' = \mathrm{d}\frac{\mathrm{d}\zeta}{\mathrm{d}r} = \Big(\frac{m + m\zeta}{m - t} - 2m\frac{m - t\zeta}{(m - t)^2}\Big)\frac{-v}{ps} = \Big(\zeta + \zeta^2 - 2\zeta^2 + \frac{2t\zeta^2}{m - t}\Big)\frac{-v}{ps} = \Big(\zeta - \zeta^2 + 2\zeta^2 . \frac{t}{m - t}\Big)\frac{-v}{ps} = \Big\{\zeta - \zeta^2 + 2\zeta^2\ (\zeta - 1)\Big\}\frac{-v}{ps} = \Big\{(2\zeta^2 - \zeta)\ (\zeta - 1)\Big\}\frac{-v}{ps} = \zeta\ (\zeta - 1) .\ (2\zeta - 1)\frac{-v}{ps}$. Now, if we suppose the temperature to vary 1° in 300 feet, we have $\frac{1}{500} \cdot \frac{1}{300} = \frac{1}{150000}$, for the variation of density depending on temperature in $\frac{1}{20900000}$ of the earth's radius x; hence t should be 139, and ζ

$= \frac{766}{766-139} = 1.26$, whence $\frac{\zeta}{mp} = 5.822$. while the phenomena of refraction require this quantity to be about 6. Thus, in Bradley's approximation, we first take $r = \frac{ps}{v}$, and then $r = p\text{ta}\left(\text{ZD} - \frac{3ps}{v}\right) = p^s\left(\frac{s}{v} - \frac{3ps}{v}(1 + \frac{ss}{vv})\right)$ very nearly, or $r = \frac{ps}{v} - \frac{3p^2s}{v} - \frac{3p^2s^3}{vv}$, and $vr = ps - \frac{3r^2}{s} - 3r^2s$, or, while s remains small, $ps = vr + 3\frac{r^2}{s}$, which is sufficiently accurate near the zenith. If we make $\frac{\zeta}{mp} = 6$, we shall have $\zeta = 1.3$, and $t = 176$, which is equivalent to a depression of a degree of Fahrenheit in 227 feet: we shall then have, for ζ', $-1.3 \times .3 \times 1.6 \frac{v}{ps} = 24 - .624 \frac{v}{ps}$, and $\frac{d^2v}{dr^2} = (1.3 - .624) \frac{v}{mp^2s^2} = .676 \frac{v}{mp^2s^2} = .676 \times 16358$, and $\frac{1}{6}$ of this, or 1854, is the coefficient of the third term. With the same value of ζ, taking $n = .15$, this coefficient would become, upon a hypothesis similar to Professor Leslie's, 2236.

8. It is not possible, in the present state of our knowledge of the subject, to determine, from observation, either the refraction with sufficient accuracy to enable us to compute from it the law of the variation of temperature, or the variation of temperature with sufficient accuracy for computing the refraction. Considering, indeed, how improbable it is that the upper regions of the atmosphere should be of the same temperature as the surface of the hills on the same general level, we could scarcely expect the agreement to be more complete than these computations make it; and it is perfectly possible either that t may be as great at 176, or that n may be .15: but we cannot determine from the observed refraction which of the laws of variation is capable of representing it with the greatest accuracy: much less should we be justified in believing, because Mr. Laplace's formula happens to represent the refraction very accurately,

that the temperature varies the less rapidly as we ascend higher. It is, however, perfectly justifiable, for the purposes of astronomy; to adopt the form of the equation which is shown by these examples to be converging, and to correct the coefficients by an immediate comparison with observation; and in this manner it has been found that the formula employed in the *Nautical Almanac* is abundantly sufficient for the purposes to which it is applied. This formula is $.0002825 = v\frac{r}{s} + (2.47 + .5v^2)\frac{r^2}{s^2} + 3600\,v\,\frac{r^3}{s^3} + 3600\,(1.235 + .25\,v^2)\frac{r^4}{s^4}$; its results are almost identical with those of the French tables, except in the immediate neighbourhood of the horizon. But the effect of a difference of temperature, at the place of observation, is not so correctly represented by any of the tables commonly employed, and requires to be separately examined.]

9. The terrestrial refraction may be most easily determined by an immediate comparison with the angle subtended at the earth's centre, the fluxion of which is $\frac{u\,dx}{vx}$, and $\frac{u\,dx}{vx\,dr}$ is initially the first part of the coefficient of the second term of the series already obtained, and is equal to [about] 6; so that this angle, while it remains small, is six times the refraction: commonly, however, the refraction in the neighbourhood of the earth's surface is somewhat less than in this proportion.

10. The effects of barometrical and thermometrical changes may be deduced from the fluxion of the equation, if we make *m*, *p*, and *n*, or rather *t*, vary: and for this purpose it will be convenient to employ the form $ps = vr + \left(\frac{1}{2\,(m-t)\,ps} - \frac{s}{2}\right) r^2$, the value of the fraction, if we neglect the subsequent terms, becoming 3.41; and this expression is sufficiently accurate for calculating the whole refraction, except for altitudes of a few degrees. Now the fluxion of $p = v\frac{r}{s} + \left(\frac{1}{2\,(m-t)\,p} - \frac{ss}{2}\right)\frac{rr}{ss}$, which we may call $p = v\frac{r}{s} + \left(\frac{1}{w} - \frac{ss}{2}\right)\frac{rr}{ss}$, is $dp = \left(\frac{v}{s} + \right.$

$\left(\frac{1}{w}-\frac{ss}{2}\right)\frac{2r}{ss}\right)dr-\frac{rr}{ssw}\left(\frac{dm-dt}{m-t}+\frac{dp}{p}\right)$, the coefficient of *dr* being equal to $\frac{2p}{r}\frac{v}{-s}$; and $\left(2p-\frac{rv}{s}\right)\frac{dr}{r}=\left(p+\frac{rr}{ssw}\right)\frac{dp}{p}+\frac{rr}{ssw}\left(\frac{dm-dt}{m-t}\right)$; $\frac{1}{w}$ being 3.4.1, and $m-t$, on this supposition, 519. The proportional variation of p, or $\frac{dp}{p}$, will be $\frac{1}{500}$ for every degree that the thermometer varies from 50°; and $\frac{dm}{m}$ being also $\frac{1}{500}$, $\frac{dm}{m-t}$ will be $\frac{766}{519\times 500}=.003$. The variation of t can only be determined from conjecture; but supposing the alteration of temperature to cease at the height of about 4 miles, it must increase, with every degree that the thermometer rises at the earth's surface, about $\frac{1}{120}$, and $\frac{dt}{t}$ being $\frac{1}{120}$, $\frac{dt}{m-t}$ will be $\frac{247}{519\times 120}=.004$. The alterations of the barometer will affect p only, $\frac{dp}{p}$ being $\frac{1}{30}$ for every inch above or below 30. It is evident, since $m=\frac{3958\times 5280\times 12}{13.57\,hd}$, h being the height of the barometer, and d the bulk of air compared to that of water, that m must diminish, as well as p, when the temperature increases; and the correction for t being subtractive, the three variations will co-operate in their effects; but the proportion will be somewhat different from that of the simple densities. If we preferred the expression derived from Professor Leslie's hypothesis, we should merely have to substitute $\frac{2dn}{1+2n}$ for $\frac{dt}{m-t}$, and the variation depending on the *law* of temperature would become about $\frac{2}{3}$ as great. It must, however be limited to such changes as affect the lower regions of the atmosphere only, its "argument" being the deviation from the mean temperature of the latitude; but even in this form it cannot be satisfactorily applied to the observations at present existing; although it appears to be amply sufficient to explain the irregularities of terrestrial refraction, as well as the uncommon increase of horizontal refraction in very cold countries: and we may even derive from all these considerations a correction of at least half a second, or perhaps of a whole second, for the sun's altitude at the winter solstice, tending to remove the discordance, which has so often been found, in the results of some of the most accurate observations of the obliquity of the ecliptic.

ii. *Extracts from two Papers on Refraction, by the Rev.* John Brinkley, D. D., *published in the Transactions of the Royal Irish Academy.*

I. Read *May*, 1814.

12. As it is of considerable importance, particularly with a view of comparing observations made in different places, that the same refractions should be generally used, no objection, I apprehend, can be made to the general adoption as far as about 80° of the French refractions, which are now so well known.

13. Perhaps the following tables, "deduced from the above formula," may be considered rather more convenient in many instances than the French tables; they will certainly furnish a useful check. The advantage they afford is derived from the facility with which the computation can be made by help of tables of logarithms and of logarithmic tangents to four or five places of figures, such as are in the "Tables requisite to be used with the *Nautical Ephemeris*." By these the logarithmic tangent of the zenith distance can be taken out at once, and the inconvenience of proportioning for the minutes of zenith distance avoided, which is greater than the new inconvenience occasioned by the second table. Hence the tables here given may be considered more convenient for observations of the sun, moon, and planets.

II. Read *January*, 1820.

Although observations of zenith distances, when the object is near the horizon, are frequently affected by irregularities of refraction, that seem not capable of being reduced to any law, yet there is reason to suppose that the effect of these irregularities will disappear in a mean of a great number of observations; and thus a mean refraction for any altitude may be obtained, depending only on the mean zenith distance, and the corresponding heights of the barometer and thermometer. The investigation of the law of these *regular* refractions, as they may be called, has much engaged the attention of astronomers.

This inquiry has led to the extremely complex but elegant mathematical researches of Kramp, Laplace, and Bessel. Their investigations are nearly related to each other. Dr. Young has also recently, by an entirely different method, and with great analytical skill, obtained an equation expressing the relation between the refractive force of air and the refraction at any zenith distance.....

It is the object of this paper to deduce, by help of a modification of the result of the hypothesis of a density decreasing uniformly, by an extremely simple investigation, the refraction, at any low altitude *corresponding to any heights of the barometer and thermometer.* The tables thence resulting, for zenith distances, between 80° and the horizon, will, I conceive, be found as convenient as can be desired. They scarcely yield in simplicity to the French tables, and enable us to obtain the quantity of refraction, as changed by the weight and temperature of the atmosphere, in which, near the horizon, the French tables appear entirely to fail.

The first tables in which this has been attended to, as far as the horizon, if I mistake not, were those of Mr. Bessel.

In our ignorance of the law of variation of density, we can only verify any hypothesis that we adopt, by a comparison of its results with those obtained by direct observation. In this way, by help of Dr. Bradley's observations, Mr. Bessel has obtained a modification of the law of uniform temperature, that will give the refractions, to within about three degrees of the horizon, with great exactness. Dr. Young has, by "adopting" a law of variation of temperature advanced by Professor Leslie, obtained an equation for refraction, the solution of which gives the refractions with considerable exactness as far as the horizon.

The following method is derived from the formula, obtained in the hypothesis of a density decreasing uniformly. However great, within the usual limits, we may suppose the change of density at the surface to be, there is no reason to suppose a material change in the *law* of density in the atmosphere. (Note. This reasoning may be fallacious, and it appears to be very desirable, that the facts should be ascertained by a sufficient number of observations, at given zenith distances near the horizon.) At present we have not sufficient observations to determine, whether the actual variations of refractions at low altitudes are most conformable to the theory of Mr. Bessel, to that of Dr. Young, or to that above given.

TABLE I.

Z. D. ° '	Log.	Diff. for 1'.	Z. D. ° '	Log.	Diff. for 1'.
80.0	1.3605	6.90	87.20	1.7837	14.55
81.0	1.4031	7.62	87.40	1.8128	14.65
82.0	1.4488	8.21	88.0	1.8421	15.50
83.0	1.4981	9.07	88.20	1.8731	16.05
84.0	1.5524	10.13	88.40	1.9052	16.75
85.0	1.6132	11.00	89.0	1.9387	17.25
85.30	1.6462	11.37	89.20	1 9732	18.00
86.0	1.6803	12.07	89.40	2.0092	18.60
86.30	1.7165	13.23	90.0	2.0464	
87.0	1.7562	13.75			

TABLE II.

Far. Therm.	Logarithms	Far. Therm.	Logarithms	Far. Therm.	Logarithms
10°	.3283	34°	.3048	58°	.2827
11	.3273	35	.3039	59	.2818
12	.3263	36	.3030	60	.2809
13	.3253	37	.3020	61	.2800
14	.3243	38	.3011	62	.2791
15	.3233	39	.3001	63	.2782
16	.3223	40	.2992	64	.2773
17	.3213	41	.2983	65	.2764
18	.3203	42	.2974	66	.2755
19	.3193	43	.2965	67	.2746
20	.3183	44	.2956	68	.2737
21	.3173	45	.2946	69	.2728
22	.3163	46	.2937	70	.2720
23	.3154	47	.2928	71	.2711
24	.3144	48	.2919	72	.2703
25	.3134	49	.2910	73	.2694
26	.3124	50	.2900	74	.2685
27	.3114	51	.2891	75	.2677
28	.3105	52	.2881	76	.2668
29	.3095	53	·2872	77	.2660
30	.3086	54	.2863	78	.2652
31	.3076	55	.2854	79	.2644
32	.3067	56	.2845	80	.2636
33	.3058	57	.2836	81	.2627

TABLE III.

Therm. near Barom.	Logarithms
20°	.2913
30	.2909
40	.2904
50	.2900
60	.2896
70	.2891
80	.2887

USE OF THE TABLES.

Log. A. in minutes = Tab. I. + (A. C.) Tab. II. + Tab. III.

Log. B. in minutes = Tab. I. + Tab. II. + Log. bar. + 7.2773.

Log. Refr. in seconds = Tab. II. + Log. bar. + log. tan. (zen. dist. − A + B).

EXAMPLE.

App. Z. D.=87° 42′ 10″. Bar. 29.50. Therm. 35°.

Tab. I.	87° 42′17	1.8160	Tab. I.	1.8160
Tab. II.	35° A. C.	9.6961	Tab. II.	0.3039
Tab. III.		0.2906	Log. Bar.	1.4698
A	63′,49	1.8027	Const.	7.2773
			B 7′,36	0.8670
			87°. 42,17	
			87 . 49,53	
			63,49	
		11.2481 Tang.	86 . 46,04	(Z.D – A + B

Tab. II.	0.3039
Log. Bar.	1.4698
Refr. 1051″,5	3.0218
= 17′ ,31″5	

[*By the Nautical Almanac.*

Alt. 2° 17′ 50″

2° 20′	R.	17′. 0″	Diff. 1′ 4,1	B. 35	Th. 2,8
		8,9	2′,10″+8,9	50,–17,5	15°+42,0
		42			
		17.50,9			
		17,5			

17.33,4 : and if 48°, instead of 50°, were the standard temperature of the table, it would be 17′. 27″,8.]

The mean of forty-two observations of α Lyrae S. P. made at the Observatory of Trinity College, Dublin, (mean of bar. 29,50, and mean of therm. 35°) gave 17′ 26″,5 [: 5″ less than Dr. Brinkley's table ; 7″ less than the N. A.].

If Table 2, Vol. XII., be added to them, they will serve for computing the refraction from the zenith to the horizon.

TABLE 2. Vol. XII.

Z. D.	BAROMETER				
	28,50	29,00	29,50	30,00	30,50
°	″	″	″	″	″
80	10,5	10,7	10,9	11,1	11,4
79	8,1	8,3	8,5	8,7	8,9
78	6,3	6,4	6,6	6,7	6,9
77	5,1	5,2	5,3	5,4	5,6
76	4,1	4,2	4,3	4,4	4,5
75	3,4	3,4	3,5	3,6	3,7
74	3,0	3,0	3,1	3,1	3,2
73	2,5	2,5	2,6	2,6	2,6
72	2,1	2,1	2,2	2,2	2,2
71	1,8	1,8	1,9	1,9	1,9
70	1,5	1,5	1,5	1,6	1,6
69	1,3	1,3	1,3	1,4	1,4
68	1,2	1,2	1,2	1,2	1,2
67	1,0				1,0
66	0,9				0,9
65	0,8				0,8
64	0,7				0,7
63	0,6				0,6
62	0,6				0,6
61	0,5				0,5
60	0,5				0,5
58	0,4				0,4
56	0,3				0,3
54	0,3				0,3
52	0,2				0,2
50	0,2				0,2
45	0,2				0,2
40	0,1				0,1
30	0,0				0,0
0	0,0				0,0

[Tab. II. + log. var. + log. tan. Z.D. = log. R′. R′ − Tab. 2. = Refr.]

Example. Z. D. 71° 26′. Bar. 29.76 i. Ther. 43°.

Log. Tab. II.	0.2965	Appr. Refr.	175″,4
Log. bar.	1.4736	Tab. 2.	2 ,0
Log. tan. 71° 26	0.4738	Refr.	173 ,4 = 2′ 53″,4
Log. appr. R. 175″,4	2.2439		

[The same Example by the Nautical Almanac.]

Alt. 18° 34′
Alt. 19° Refr. 2′.47″,7 Diff. Alt. ,16 B. 5,61 Th. ,34
+ 5,2 − 26′ = +4,16 .24, −1,34 7°, +2,38
2′.52″,9 1,34
2.53,4 1,04
Difference ,5.]

iii. *Observations on* M. Delambre's *Remarks, relative to the Problem of finding the Latitude from two Altitudes, and the Time between. By the* Rev. JOHN BRINKLEY, D.D., Professor of Astronomy in the University of Dublin.

In giving, in the last number of the Astronomical and Nautical Collections, M. Delambre's method of finding the latitude from two altitudes of the sun and the time between, a remark of his was inserted, containing a fundamental objection against the method of Douwes. It appears, however, that M. Delambre, in his *Nouvelles Réflexions, Conn. des Tems*, 1822, p. 316, has pursued an erroneous line of reasoning, a circumstance rare indeed as to that learned and illustrious astronomer. That objection to the indirect method of solving the problem is not founded.

M. Delambre has the equation (p. 317), $\frac{a}{c} = \cos. \mathrm{H} - \tan. \psi \sin. \mathrm{H}$, ψ being a small arc. He assumes a value for H, and computes both ψ and $(\mathrm{H} + \psi)$ from this same equation, and finds $(\mathrm{H} + \psi) - \psi = \mathrm{H}$. This surely could not be otherwise. It is singular that it escaped M. Delambre, that the interval between the observations disappears from his equation.

Another point of view shows, that what he has done is not relative to the method of Douwes. It is not an impossible supposition to make the declinations exactly equal, and then his method of computation, page 321, concludes nothing, instead of becoming the method of Douwes.

The usual objections to the method of Douwes are, 1. Not allowing for the change of declination. This can occasion no error of consequence to the navigator. But in fact, if great accuracy be desired, the change of declination may be easily allowed for in practising the method of Douwes, as stated below. 2. That, except under great limitations, the latitude computed may be further from the truth than that by the reckoning. But Dr. Brinkley's method entirely obviates this objection. His method is intended to correct and extend the results obtained by the original method of Douwes, even for cases where it would otherwise be quite useless. 3. The length of the computation has also been objected to, but unless it be repeated two or three times, it is shorter than the direct can be made, and it possesses no ambiguity embarrassing to those not conversant in spherical trigonometry. By Dr. Brinkley's method it rarely indeed happens that two operations are necessary.

Dr. Brinkley has given the following method of allowing for the change of declination.

Having computed the middle time by the method in the requisite tables, or by the common log. tables, add to it half the interval to get the time furthest from noon. *Or* use the estimated time when the observation furthest from noon was made; add together (three places of log. are sufficient), the sine of time furthest from noon, the secant of the altitude belonging to this time, and the cosine of the lat. by account; look for the sum among the log. sines, and take out the corresponding cosine, which is to be added to the log. of the change of declination in minutes. The sum is log. of the correction of the altitude furthest from noon. This is to be added to that altitude when the sun at the other observation is nearer the elevated pole; otherwise subtracted. The altitude so corrected is to be used instead of that observed, and the declination to be used is that at the observation nearest noon. The computed latitude found is to be corrected by Dr. Brinkley's rules in the *Nautical Almanac* 1822.

It will rarely occur that the time is not known with sufficient exactness for this correction of the altitude made to obviate the effect of the change of declination. Thus the correction will be easily had for the direct method.

M. Delambre's example, thus computed, will afford an instance of the indirect method leading to a true result, when exactly computed; contrary to the opinion of M. Delambre.

EXAMPLE.

```
   1st. Ob. PM. alt. 42° 14′,1 }   Interval   Decl. 8°.15′ N.             Lat. by reckoning
   2    Ob. PM. alt. 16   5,8  }   3h=45°     Increase of declin.=3′      48°.45′ N.

                                     +-------------------------------------------+
                                     |nat sin. 42 14,1  67217                    |
 Sec. 8°.15′ . . . . 10.00452        |         16  5,8  27726                    |   log. 3′  0.477
 Sec. 48.45 . . . . . 10.18089       |                  -----                    |            9.875
                      4.69897        |                  39491   log. 4.59649     |          -------
                     --------        |                               0.41716     |    2′,3   0.352
            A         4.88438        |                          A    4.88438     |  16 5,8
  AC sin 22° 30′      0.41716        |                              --------     |  ------
  39555 log           4.59720        |       med. time 52°.15′  sin 9,89803      |  16 3,5
                     --------        |                 22. 30                    |
med. t. 52°22′,5 sin  9.89874        |                 ------                    |
      22 30                          +-------------------------------------------+
     ------                              t. fur. from noon 74.45  sin 9.984
t.n. noon 29 52,5                                          16. 6  sec 10.017
     ---------           net sin 4214,1 67217              48.45  cos  9.819
      14 56,2  sin.  9.41119     net sin 16 3,5 27662                  -------
                           2                 -----                 sin  9.820
                    --------                 39555                     -------
                     8.82238                                       cos  9.875
             A       4.88438
                    --------
      8670   log     3.93800          cot 48°45 9.993        tan 52°.22′     10.113
      67217                           tan  8 15 9.161        cot. 14 .56     10.574
      -----                                     -----                         ------
      75887 sin 49°.21′,8                   2)19.104                    2)20.687
                ---------                   P sin  9.552              Q   tan 10.343
                 40  38,2                          -----                       ------
                  8  15                     + L cos  9.970              sec tan 10.293
                 -------                    — T     10.146                          150
 lat. compd.     48  53,2                           ------           14°.56′ sin  9.411
 lat. by acc.    48  45                     — S tan 10.116                        ------
                 -------                            ------                         9.854
        + D       8,2    log 0.914          — C sec 10.216                        20.000
                             9.558                       2                        ------
                             -----                  ------                   — T 10.146
   Corr. lat. — 3, 0         0 482                   0.43-2
              4853,2                                ------
              ------                        A.C.     9.568
   Lat.      48.50,2                                ------
```

Had the estimated time been used, the enclosed part would have been unnecessary.

Taking 48° 55 for the lat. by account, the second supposition of Delambre, the computed lat. will be 48° 47′2

```
                48 55
               -------
   — D          7,8 log. 0.892
                         9.568
                         -----
                         0.460
   Corr. lat. + 2′,9
               48 47,2
               -------
   Lat.        48 50,1
```

```
Supposing lat. by reckoning 47°.50′          This lat. is inexact only by 1′,
Lat. computed will be        49  23,6          although the lat. by account
                             --------          was inexact by 1°.
                             1  33,6=
         + D                   93,6 log 1.971
                                        9.568
                                        -----
         Corrn lat.            34′,6    1.539
                             49 23,6
                             -------
         Lat.                48 49,0
```

IV. *Vindication of the* Connaissance des Tems, for 1812.

It appears from an Article in the *Annales de Chimie* for April, that the error of the Table of Corrections of the places of the stars, in the *Connaissance des Tems for* 1812, consists only in the omission of the character ⊙ at the head of the second column. This omission had led two astronomers of considerable reputation in London to point out the whole table to the Editor of these Collections as erroneous; and he is obliged to confess, that although he suspected the nature of the error, he had NOT the sagacity to discover how simply it might be *remedied*, as perhaps he ought to have done.

He had himself been put to great trouble and inconvenience for want of the errata page of the *Connaissance des Tems for* 1823, having received the volume without it: and he thought it due to the Editors to endeavour to supply the deficiency of their Booksellers or their Binders: never imagining that they could have supposed him so mean spirited, as to mention the circumstance from jealousy or ill nature, or that they could have attributed to him the silly vanity of seeking to claim reputation from having been the humble instrument of correcting a few errors of the press. That he was not deficient in sincere respect for the author of the table, or in gratitude for the labours of the French Astronomers, is sufficiently demonstrated by his remarks subjoined to the Lunar Observations computed and compared: and the many marks of friendship and kindness, which he has received from the Editors of the *Annales de Chimie*, have rendered it impossible that he should voluntarily have made any observations, that he could have supposed likely to wound their feelings unnecessarily. He might indeed have fancied, that he had some little reason to complain, that no acknowledgment was made, in the errata page in question, of the source from which it had been derived: and still more that no return had been made for the communication, by a private indication of a similar nature. He has now, however, for the first time, to acknowledge a favour of this kind, in a public denunciation of no less than "60 errors" at once; to which he must himself add, extempore, 180 more, of equal magnitude, beginning with the year 1821; the English computers having *always* neglected to attend to the observation of Mr. Burckhardt, contained in a note at the end of his Tables, that the Supplement of the node is to be diminished by 7′ whenever it is to be inserted in an Almanac. It may, however, be remarked, that this omission can never have a *sensible* effect in any computations, for which the mean place of the node, as set down in the Almanac, is employed; and that both these misconceptions might have been easily avoided, if the learned author of the tables had condescended to give a single example, of the manner in which a computer is to proceed, in employing every part of them. But it must be confessed, that it is difficult for a real mathematician to be aware of all the precautions, that are required, for avoiding the occurrence of errors of this sort in the hands of mere mechanical labourers.

v. *The Force of Magnetism, compared with the Dip. Extracted from* Captain SABINE's *Appendix to* Captain PARRY's *Journal. 4to. London,* 1821, p. cxxxviii.

" Having detailed the Observations on the intensity of the Magnetic Force, it may not be uninteresting briefly to examine, how far the results are consistent with the ratio in which it was expected that the magnetic force would be found to vary under different dips of the needle.

" In the Rules and Tables for clearing the Compass from the regular Effect of the Ship's Attraction, printed in 1819 by order of the Commissioners of Longitude, and published, with some alterations and additions, in the *Journal of the Royal Institution* for October, 1820, the magnetic force in the direction of the dipping needle is considered to vary, inversely, as the square root of four diminished by three times the square of the sine of the dip; and the force acting on a needle limited to a horizontal motion, inversely, as the square root of three increased by the square of the secant of the dip.

" The Observations at Melville Island are entitled to principal consideration, as having been made under more favourable circumstances than were presented by the other opportunities of the voyage; they are, therefore, to be compared with those which were made in England.

"The dip in London being 70° 33′.3, and at Winter Harbour 88° 43′.5, the force in the direction of the dipping needle should increase by calculation in the ratio of 1.153 to 1.

"The time of vibration of Mr. Browne's dipping needle decreased, between London and Winter Harbour, in the proportion of 481 to 446, and consequently the force appeared to have increased in the ratio of 1.163 to 1.

"The dip at Sheerness being 69° 55′, and at Winter Harbour 88° 43′.5, the magnetic force should increase by calculation as 1.163 to 1; but the force acting on the horizontal needle should be diminished in the proportion of 13.275 to 1.

"The times of vibration of the three horizontal needles increased between Sheerness and Winter Harbour, in arcs from 7 to 14 degrees, respectively, as follow: No. 1 as 339.7 to 94.5; No. 2 as 327.4 to 90; and No. 3 as 316.1 to 85; consequently, the force acting on them appeared to have diminished by No. 1 as 12.93 to 1; by No. 2 as 13.23 to 1; and by No. 3 as 13.83 to 1; the mean being as 13.33 to 1; differing but $\frac{1}{230}$ from the result of the calculation.

"This is, perhaps, a nearer agreement with the theory than there was reason to have expected, considering how much the unavoidable causes of uncertainty in such experiments are augmented in high magnetic latitudes.

"The results on the 26th of June, and on the 23d and 24th of July, 1819, compared with the observations in England, will also be found to agree as well with the theory as it is reasonable to expect in experiments, where neither time nor circumstances admitted the adoption of the precautions, requisite to ensure the utmost accuracy of which they are capable; and where, perhaps, the moving of the ice during the observations may have introduced an additional error which no care could guard against. By the experiments of the 26th of June, the force had diminished by Needle No. 2, as 2.815 to 1, and by No. 3

as 2.86 to 1; and supposing the Dip to have been 83° 04′, as was found by rather an indifferent observation, the theory would require a diminution of 2.5 to 1. By the experiments of July the force had diminished by Needle No. 2, as 3.28 to 1, and by No. 3, as 3.198 to 1; the calculated diminution being 3.12 to 1."

It does not appear that this connexion between the dip and the magnetic force was ever before theoretically laid down, or experimentally established; it is, however, only an application of the system of Aepinus and Coulomb to a hypothesis respecting the magnetism of the earth, which is perhaps partly original, but which is the most simple and natural that could be assumed.

A strange assertion has lately been made, in a periodical publication, respecting the author of the paper in question, which is, that he must have been ignorant of the existence of a plane, in which a mass of iron, rendered magnetic by its temporary situation with respect to the earth only, produces no effect in the needle of a compass placed near it.

It is impossible that such an assertion could have been made by one who had properly studied the grounds of the modern theory of magnetism, as laid down by Aepinus, by Dr. Robison, by Haüy, by Biot, or by any other good elementary writer, and who had read the paper with any thing like attention.

The most superficial consideration of the nature of induced magnetism will show, that a mass of soft or conducting iron must become "a terrella," or earth in miniature, by the action of the earth's magnetism; and that as such, it must have its magnetic equator, on which the direction of the magnetic force is parallel to that of its magnetic axis, and consequently to the magnetic axis of the earth; and that when the needle is situated in the plane of this equator, it cannot be disturbed by the magnetism of the terrella, which must act in the same line as the force of the earth itself, though in a contrary direction.

That Mr. Lecount should have fancied this a new discovery is not at all surprising; and it does him great credit to have pursued his investigation under many disadvantages; but it is much more remarkable that a literary man, who has written a great deal, and who has probably read a little, should have been so completely uninformed.

If the author of the paper published in these selections had been ignorant of the fact, he could never have remarked, in the 11th section, that "a horizontal bar of soft iron will lose its effect on the needle in four positions, at right angles to each other; and a bar so inclined as to become perpendicular to the dipping needle in the plane of the meridian, will lose its effect in two opposite positions in that plane only;" nor could he have obtained the result now so strikingly verified by Captain Sabine, if he had been so ignorant of the theory as the Reviewer's assertion implies.

It is true, that, if the account of Captain Flinders's observations is correct, the table given in this paper is not applicable to such a ship as Captain Flinders's, in the neighbourhood of the equator, nor in the southern hemisphere; and that if the effect which was simply called *regular* in the first *unpublished* impression of the paper, had been considered by the seamen intrusted with it for trial, as the principal or only effect, though the author distinctly pointed out another effect, as frequently occurring, under the name of the *irregular* attraction, they might have been misled by the incautious application of the table to all cases indiscriminately.

It is also true, that Mr. Barlow and Mr. Lecount have ascertained, that cast iron is capable of producing more distinct effects, by its induced magnetism, than might have been expected by those who understood the term of *soft* iron, as synonymous with conducting, in too literal a sense: and if Mr. Barlow had claimed this as a discovery, and if he had also claimed the merit of having first experimentally ascertained the truth of his very ingenious friend Charles Bonnycastle's theoretical assertion, that the induced magnetism of a shell ought to be equal to that of a solid sphere; it would have become the duty of those, who sat in judgment upon his papers, to examine how far these discoveries were actually altogether original, and how far they deserved to be announced to the world as such: but if such a publication could not be encouraged without admitting the originality of other facts, which they supposed to be known to every student of natural philosophy, they had surely some reason to hesitate, before they gave it their unqualified approbation.

vi. *Third Report of the Commissioners appointed by His Majesty to consider the subject of Weights and Measures.*

MAY IT PLEASE YOUR MAJESTY,

WE, the commissioners appointed by your Majesty, for the purpose of considering the subject of weights and measures, have now completed the examination of the standards which we have thought it necessary to compare. The measurements which we have lately performed, upon the apparatus employed by the late Sir George Shuckburgh Evelyn, have enabled us to determine, with sufficient precision, the weight of a given bulk of water, with a view to the fixing the magnitude of the standard of weight; that of length being already determined by the experiments related in our former Reports: and we have found by the computations, which will be detailed in the Appendix, that the weight of a cubic inch of distilled water, at 62° of Fahrenheit, is 252.72 grains of the Parliamentary standard pound of 1758, supposing it to be weighed in a vacuum.

We beg leave therefore finally to recommend with all humility, to your Majesty, the adoption of the regulations and modifications suggested in our former Reports; which are principally these:

1.—That the parliamentary standard yard, made by Bird in 1760, be henceforwards considered as the authentic legal standard of the British empire; and that it be identified by declaring, that 39.1393 inches of this standard, at the temperature of 62° of Fahrenheit, have been found equal to the length of a pendulum supposed to vibrate seconds in London, on the level of the sea, and in a vacuum.

2.—That the parliamentary standard troy pound, according to the two pound weight made in 1758, remain unaltered; and that 7000 troy grains be declared to constitute an avoirdupois pound; the cubic inch of distilled water being found to weigh at 62°, in a vacuum, 252.72 parliamentary grains.

3.—That the ale and corn gallon be restored to their original equality, by taking for the statutable common gallon of the British empire, a mean value, such that a gallon of common water may weigh 10 pounds avoirdupois in ordinary circumstances, its content being nearly 277.3 cubic inches; and that correct standards of this imperial gallon, and of the bushel, peck, quart and pint derived from it, and of their parts, be procured without delay for the Exchequer, and for such other offices in your Majesty's dominions, as may be judged most convenient for the ready use of your Majesty's subjects.

4.—Whether any further legislative enactments are required, for enforcing a uniformity of practice throughout the British empire, we do not feel ourselves competent to determine. But it appears to us, that nothing would be more conducive to the attainment of this end, than to increase, as far as possible, the facility of a ready recurrence to the legal standards, which we apprehend to be in a great measure attainable by the means that we have recommended; it would also, in all probability, be of advantage to give a greater degree of publicity to the Appendix of our last Report, containing a comparison of the customary measures employed throughout the country.

5.—We are not aware that any further services remain for us to perform in the execution of the commands laid upon us by Your Majesty's commission; but, if any superintendence of the regulations to be adopted were thought necessary, we should still be ready to undertake such inspections and examinations, as might be required for the complete attainment of the objects in question.

London, (Signed) GEORGE CLERK.
31 *March*, 1821. DAVIES GILBERT.
W. H. WOLLASTON.
THOMAS YOUNG.
HENRY KATER.

APPENDIX.

THE commissioners having been furnished, by the kindness of the Honourable Charles C. C. Jenkinson, with the apparatus employed by the late Sir George Shuckburgh Evelyn, in the determination of the magnitude of the standard weights, and there being some doubt of the perfect accuracy of his method of measuring the capacity of the bodies employed, it was judged necessary to repeat that measurement with greater precautions; and the results of Captain Kater's experiments have afforded some slight corrections of the capacities in question.

The sides of Sir George Shuckburgh's cube were found by Captain Kater equal to 4.98911, 4.98934, and 4.98935 inches; the diameter of the cylinder 3.99713, and and its length 5.99600 inches; and the diameter of the sphere 6.00759 inches. Hence the content of the cube appears to be 124.1969 inches; that of the cylinder 75.2398; and that of the sphere 113.5264 inches of Bird's parliamentary standard of 1760, recommended in the last Report of the commissioners, or of the standard made by Troughton for Sir George Shuckburgh.

The difference of the weight of the cube in air at 62°, with the barometer at 29.0, and in water at 60.2°, was 31281.79 grains; and adding to this the weight of an equal bulk of the air at 62°, which is $\frac{1}{834}\cdot\frac{29}{30}$ of that of the water, or 36.26 grains, and subtracting from it $\frac{1}{8.5}$ of this, or 4.26 grains, the buoyancy of the brass weights, we obtain 31413.79 grains for the weight of the cube of water in a vacuum at 60.2°. Now this cube is less than the supposed measure at the standard temperature of 62°, in the ratio of 1 to 1.0000567, on account of the contraction of the brass; and the water is denser than at the standard temperature, according to Mr. Gilpin's experiments, in the ratio of .99998 to .99981, or of 1.00017 to 1, the whole correction, for the difference of 1.8°, being .0001133, or 3.55 grains, making 31410.24 for the weight of the cube of water in a vacuum at 62°; which, divided by 124.1969, gives 252.907 for the weight of a cubic inch in Sir George Shuckburgh's grains.

In the same manner we obtain for the cylinder, which was weighed in air under the same circumstances, and in water at 60.5°, the difference being 19006.83 grains, the correction $\frac{1}{834}\cdot\frac{29}{30}\cdot\frac{7.5}{8.5}$ for the effect of buoyancy, amounting to 19.43 grains, and for the difference of temperature of the water and brass conjointly, the densities being .999955 and .999810, the correction .000145 − .000047 = .000098, or 1.86 grains, leaving + 17.57 grains for the whole correction of the weight, as reduced to a vacuum at 62°, and making it 19024.40, which divided by 75.2398, the content of the cylinder, affords us 252.851, for the cubic inch in a vacuum at 62°.

The sphere was weighed in air at 67°, the barometer standing at 29.74; the correction for buoyancy is here $\frac{7.5}{8.5}\cdot\frac{29.74}{30}\cdot\frac{1}{843}$, or for 28673.51 grains, 29.72; while the temperature of 66° requires, for the difference between the expansion of brass and water, the addition of .00042 − .000126, or .000294 of the whole, that is + 8.43 grains, making the whole correction 38.15, and the weight in a vacuum 28711.66; which, divided by 113.5264, gives us 252.907, for the cubic inch in a vacuum.

The mean of these three measures is 252.888, giving for the three errors + .019, —.037, and + .019; and this mean, reduced to the parliamentary standard, makes 252.722 grains, for the cubic inch of distilled water at 62°, weighed in a vacuum, or 252.456 in air, under the common circumstances of the atmosphere, when weights of brass are employed. In a vacuum at the maximum of density, that is at 39°, the weight of a true cubic inch will be 253 grains, and of a cubic decimetre 15440*. The proposed Imperial Gallon, of ten pounds, or 70000 grains, of water, will contain very nearly 277.3 cubic inches, under common circumstances.

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* It appears, however, from an official Report, obligingly communicated to us by Dr Kelly, that the actual standard chiliogramme has been found to contain only 15433 English grains.

ART. XVII. *Miscellaneous Intelligence.*

I. MECHANICAL SCIENCE.

§ THE ARTS, AGRICULTURAL ECONOMY, &c.

1. *Improvement of Oil Lamps.*—MM. Arago and Fresnel have lately applied the principle of Count Rumford's concentric or co-lateral meshes to the improvement of lamps, intended either for light-houses or theatres, or for other uses where a strong bright clear light is wanted. In order to obviate the difficulty which was formerly found to arise from the carbonization of the wick by the great heat occasioned at the summit of the burner, the oil was made to flow over at the mesh, in the manner proposed and adopted by M. Carcel; and in thus keeping the flame at the top of the wick, a full, clear and steady combustion was obtained. Many circumstances require attention in making these lamps produce their best effect; as the space between the meshes, the size of the air canals, the height of the chimney, the magnitude of the reservoir, &c. When perfect, the experiments made with them, though they seemed to indicate a slight degree of saving in the oil required to produce a certain quantity of light with lamps having two wicks, yet they did not, as a general result, with three and four wicks, justify the opinions of Count Rumford; and the quantity of light produced, was about the same as what would have been given by the same quantity of oil burned in other economical methods. The principal advantage is the power of concentrating all the light into one focus, so that when advantageously placed, as on the centre of a light-house furnished with lens, the greatest quantity may pass from one point through the lenses. The light of these lamps is very regular; for, after twelve or thirteen hours' burning, it does not diminish more than one-fifth,—at least such was the result with a four-wicked lamp placed in the focus of a large lens. The quantity of oil allowed to flow over at the wick, should be at least equal to that which is burned. It is not apparently injured, and is to be returned into the reservoir. In place of the apparatus employed by M. Carcel, to make the oil rise to the wick, MM. Arago and Fresnel placed the reservoir of oil above the height of the burner; and then, by an open moveable tube, which passed into it from the air, the level up to which the oil was required to flow was easily regulated.—*Annales de Chimie*, xvi, p. 377.

2. *Coal-Oil Parish-Lamps.*—It is now some time since the volatile oil, obtained by distilling coal and coal-tar, has been applied in place of animal oil, in producing light. Large quantities of this fluid are prepared at once from the coal in

Scotland, and much is also obtained by distilling coal-tar. When pure, it is limpid and colourless, and closely resembles, if it be not identical with, naphtha. A large district about Fitzroy-square and Charlotte-street has been lighted by this fluid, burned in lamps particularly constructed for it by Major Cochrane; they are patent, as well also as the application of the oil to this purpose. The flame in these lamps is very short, but extremely bright, and certainly far surpasses a common street gas flame in that respect, if it does not also an Argand burner supplied by coal-gas. It has happened now and then, when the wick has been too high, and the oil used has been obtained from coal-tar, that the flame has smoked, the wick become charred, and at times so much vapour has collected in the lamp as at last to explode and burst it to pieces; but this has not happened with the Scotch oil. The lamps in the district before mentioned, have now been in use for a considerable time, and are found to be attended with perfect success.

3. *Lithography.*—A society has been formed at Munich for the imitation of oriental MSS.; the object is by means of lithography to multiply copies of the best works which are extant in the Turkish, Arabic, Persian, and Tartar tongues, and to dispose of them in the East, by the port of Trieste. The cabals of those, whose business it is to write MSS., and the different ornaments with which the Turks and Arabs adorn their writings, have been obstacles to this design hitherto; but by the aid of Lithography, the difficulty it is thought may be overcome. Thus the cheapness of that mode of engraving will contribute to spread to an unlimited extent, the treasures of the best writers of the East.

A lithographic establishment has also been formed in London, for the purpose of facilitating the progress of this branch of art, at No. 1, Wellington-street. Series of the impressions taken from copies of the pictures in the Munich gallery are to be seen there, and give an idea of the powers of the art, far beyond what could possibly be imagined by those who know of it only from description. It contains also a large deposit of Foreign and British Materials, for the prosecution of this pursuit, and many of the finest results that have been produced by it.

4. *On the Potash to be obtained from the Stalks of Potatoes.*

[In a Letter to the Editor.]

Dear Sir,

In *Tilloch's Journal* for November 1817, there is inserted an article on the manufacture of potash from the stalks of potatoes. The experiments are said to have been executed first in France, and the results are all given in weight, from that of the green vegetable to the incinaration of the mixed saline mass, commonly known in our markets by the name of pearlash. The "immense advantages," however, which are to be derived from this practice, seem to depend on a fallacy which is of such a nature, that it appears rather to be the consequences of a direct mis-statement than of any conceivable error. The French statement is indeed followed by a comparative experiment made in Ireland by Mr. Rice, the results of which are far different, and such as to confirm the suspicion here stated. According to the French experiment, the produce of potash per acre, is above 2,000 lbs.; in Mr. Rice's, it is only 201½ lbs. It is true that owing to differences in the method of burning vegetables, whether terrestrial or marine, they are found to yield very different proportions of alkali, from circumstances respecting the peculiar nature of these substances, the mode of their existence in vegetables, and the changes they undergo in the fire, with which we are not yet acquainted. But as the method of drying and burning are fully described in the original experiment and appear to have been accurately followed in Ireland, this cannot account for the extraordinary differences in the results respectively obtained.

Being desirous of further verifying the French statement, which, certainly, if correct, offered no small temptation to agriculturists, I requested my friend Sir John Hay, Bart., to make a large experiment for that end, on his farm near Peebles; and as it was executed with that accuracy which characterizes his whole system of agriculture, his well-known reputation will afford sufficient proof that it was worthy of reliance. I ought, however, to add, that lest any thing should arise to throw a doubt on the event, the directions given in the narrative of the French experiment, were implicitly followed in every particular, from the cutting to the burning of the plant, and that the ashes were weighed, lixiviated, and examined by myself.

The result of two trials on two separate acres, follow; and the Scotch acre, it must be remembered, is one seventh larger than the English. It is presumed that in the original statement the measures were reduced to the English acre. The first acre was a rich loamy soil at King's Meadows; the potatoes were drilled, and produced a good crop. They were cut, as directed, immediately after flowering, left ten days to dry, and burnt in a pit. the produce was 222 lbs. of ashes, and, on lixiviation and drying, these yielded 55 lbs. of impure potash, or mixed salts.

The second acre was a clayey wet soil, with a retentive bottom; but the crop, which was also drilled, was considered moderate. These stems were treated in the same manner; but the burning had been more complete, as the ashes contained less charcoal than the preceding. They only weighed 112 lbs., and produced 28 lbs. of impure potash.

Thus it appears that, in the Irish experiment, the potash procured was scarcely one-twentieth part of that which was said to have been obtained in the French; while, in the two experiments made in Scotland, the produce, in one instance, was only the half, and, in another, only the quarter of that which was obtained from the trial made in Ireland.

It is probable that the Irish acre, if computed according to the lazy-bed system of cultivation, contained more plants than the drilled Scotch acre; and thus the differences of produce between Mr. Rice's acre, and that at King's Meadows, will not be very difficult to reconcile; while the scantiness of the second crop, tried in Scotland, may also suffice to account for the still greater diminution of the alkaline product in that case. Both these trials therefore sufficiently confirm each other; particularly when we further consider the differences in the proportion of saline ingredients which the same plant exhibits in different situations and circumstances, and those further differences in the alkaline product which arise from variations in the treatment previous to burning and during combustion. The French experiment, however, leaves them at an incalculable and incredible distance.

Taking a mean result then from the experiments made in Ireland and Scotland, or even admitting the former to afford a better standard, there is evidently no temptation for agriculturists to repeat these trials with a view to profit. It appears on analysis, that the dry saline residuum, here called in compliance with the French statement, impure potash, does not contain above ten per cent of pure alkali, the remainder consisting of muriate of potash and other ingredients; and it is evident that a much larger quantity would not repay the expenses incurred in the operation,

I am yours, &c.,
J. Mac Culloch.

5. *Apple Bread.*—M. Duduit de Maizieres, a French officer of the king's household, has invented, and practised with great success, a method of making bread with common apples very far superior to potato bread. After having boiled one-third of peeled apples, he bruised them while quite warm into two-thirds of flour, including the proper quantity of yeast, and kneaded the whole without water, the juice of the fruit being quite sufficient. When this mixture had acquired the consistency of paste he put it into a vessel, in which he allowed it to rise for about twelve hours. By this process he obtained a very excellent bread, full of eyes and extremely palatable and light.—*New Monthly Mag.*

6. *New Musical Instrument.*—A musical instrument is now in

London, called the Terpodion. It is the invention of M. Buschman, who has lately brought it from the continent. Its effect is striking, and astonishing, for it combines the sweetness of the flute and clarionet, with the energy of the horn and bassoon, and yields a full and rich harmony, resembling an orchestra of wind instruments. The sounds may be continued at pleasure with any degree of strength, and the action of the instrument resembles that of the ediphone, but it is described by the inventor as consisting entirely of wood, and it is understood that the sounds are produced by the vibration of wooden staves; its construction is said to be cheap, and its state unalterable by the weather.

7. *Large reflecting Telescope.*—Mr. J. Ramage of Aberdeen has constructed a twenty-five feet reflecting telescope, the largest except that of Sir W. Herschel ever made. The speculum is twenty-five feet focal length and fifteen inches diameter, the power from 50 to 1,500, and the mechanism by which the observer and instrument are moved, is simple and well contrived.

8. *Iron Bridges.*—Carthage bridge on the Genessee river in the state of New York, fell to pieces on the second of May (1820?). It was a single arch of iron, and for its extent and height stood unrivalled in America or in Europe. The arch consisted of nine ribs, its chord 352 feet, and height of the railing above the water 200 feet; the length of the floor 714 feet.—*New Monthly Magazine.*

9. *Prize Question.*—The following question has been proposed by the Society of Sciences and Arts of Utrecht. " What relation is there between speculative philosophy and mathematics? Why are mathematics necessary to philosophy, excluding their application to physics? and what means does speculative philosophy offer for the extensive and ultimate perfection of pure mathematics?"

II. CHEMICAL SCIENCE.

§ CHEMISTRY.

1. *Permeability of Iron to Tin.*—Mr. Smithson describes, in the *Annals of Philosophy*, vol. i., p. 276, an instance where tin had been forced through the pores of cast iron. It is adduced, in support of the opinion, that the capillary copper in the slag of the Hartz, has been formed by being pressed through minute pores. " For some purposes of the arts, Mr. Clement formed a cylinder of copper, and, to give it strength, introduced into it a hollow cylinder, or tube of cast iron. To complete the

union of these two cylinders, some melted tin was run between them. With the exact particulars of this construction, I am not acquainted; but the material circumstance is that, during the cooling of the heated mass, a portion of the melted tin was forced, by the alteration of volume of the cylinders, through the substance of the cast-iron cylinder, and issued over its internal surface in the state of fibres, which were curled and twisted in various directions. Such was the tenuity of these fibres of tin, that little tufts of them applied to the flame of a candle, took fire and burned like cotton."

Mr. Smithson suggests, that perhaps this penetration of solid matter, by other solids or fluids, by great pressure, may have useful applications, and produce some compounds very advantageous in the arts.

2. *Granulation of Copper.*—The following singular circumstance is related by Mr. W. Keates, of the Cheadle copper-works, to Mr. Phillips. "I send you some globules of copper, quite hollow, and so light as to swim on water; the history of which is as follows: one of our refining furnaces contained about 20 cwt. of melted copper, which was to be laded into blocks; but the refining process had not been carried far enough, so that when the men came to lade it out into the moulds, they found it to be impracticable, in consequence of its emitting such a great quantity of sulphurous acid vapour. They were therefore obliged to put it into a cistern of water, to granulate it; but by this operation, instead of the copper assuming the form of solid grains, the whole of it became in the form sent to you, and floated on the water like so many corks. What is the most probable explanation of this phenomenon? One of our refining men, during forty years' experience in the business, has never seen any thing similar." Mr. Phillips adds, that the globules of copper sent to him were light, and that, though they had lost the power of floating on water, they floated in sulphuric acid.—*Annals of Philosophy*, vol. i. p. 470.

3. *Selenium.*—In consequence of the repairing of the leaden chamber of the sulphuric acid works, at Gripsholm, a quantity of a substance has been collected, consisting principally of sulphur, impregnated with selenium. This has been forwarded by Professor Berzelius, to Mr. Allen, of Plough-court, Lombard-street; and is to be sold in small quantities, for the benefit of the proprietors. A translation of the process recommended by Professor Berzelius, for the separation of the selenium is given with the substance.

4. *Chromic and Sulphuric Acids.*—When sulphuric acid is boiled on chromate of lead or baryta in excess, the chromic

acid obtained is not pure, but contains sulphuric acid. The liquid containing the two acids, when successively evaporated, entirely crystallizes in small quadrangular prisms of a deep red colour. If the heat and concentration be carried too far, oxygen is disengaged, and sulphate of green oxide of chromium found. These crystals are deliquescent, and contain one atom of each of the acids. To analyze them, they were boiled with a mixture of muriatic acid and alcohol, so as to convert the chromic acid into green oxide; then dividing the liquid into two parts, one was precipitated by muriate of baryta, to give the sulphuric acid; and the other by ammonia, for the oxide of chrome, and consequently the chromic acid.

Alcohol easily dissolves this substance, and, if strong, so rapid a decomposition is produced, as to resemble an explosion. The chromic acid becomes oxide of chromium, and a particular ethereal odour is produced. Having ascertained that the same odour was produced by treating peroxide of manganese with alcohol and sulphuric acid, I collected some of this ethereal fluid by distillation, and rectified it on lime to separate water, and on chloride of calcium to separate alcohol. It was then of an acrid burning taste, and very penetrating odour, resembling sulphuric ether. When mixed with water, it separated into a stratum of sulphuric ether, and a white transparent light oil, identical with the sweet oil of wine. The mixture of alcohol, sulphuric acid, and black oxide of manganese, that had been used, contained much sulphate of manganese, but no hyposulphuric acid.

Hence, in treating alcohol by chromic and sulphuric acid, or by the latter and peroxide of manganese, it appears to undergo the same alteration, as by sulphuric acid alone. Sulphuric ether and sweet oil of wine are formed by means of the oxygen of the chromic acid, or of the peroxide of manganese. The sulphuric acid suffers no alteration, but its presence is necessary to determine the decomposition of the alcohol and the partial deoxidation of the chromic acid, or peroxide, in consequence of its affinity for the oxides of chromium and manganese. I do not doubt but that it might be replaced by many other acids.

M. Gay Lussac, to whom these experiments are due, then notices, that Scheele and Dobereiner had noticed effects relative to this subject: Scheele remarked the ethereal smell, *&c.*, produced by the action of peroxide of manganese, sulphuric acid and alcohol, and distilling slowly; and Dobereiner had observed a similar odour in a mixture of chromate of potassa, sulphuric acid, and alcohol.—*Annales de Chimie*, vol. xvi. p. 103.

5. *Native Carbonate of Magnesia.*—Dr. Henry has published an account of a native carbonate of magnesia, brought from the

East Indies by Mr. Babington. It occurs massive; its colour is snow white, with the exception of a few small dots and stripes of ochre-yellow; its fracture is small conchoidal, passing into uneven; it gives sparks with steel, is not easily scraped by a knife, but is not hard enough to scratch fluor spar; its fragments are sharp-edged. Internally it has no lustre; it is very slightly transparent, and that only at the edges. Its specific gravity is 2.5615; its locality is not known.

It dissolves in acids slowly when cold, even though powdered; but heat quickens the solution, and its accompanying effervescence. When analyzed, 100 grains gave

Magnesia	46
Carbonic acid	51
Insoluble matter	1.5
Water	0.5
Loss	1.

so that it is nearly a pure carbonate of magnesia.

Dr. Henry, at the conclusion of this analysis, expresses his doubts of the existence of a true bi-carbonate of magnesia. They are founded on the analysis of a salt prepared by himself, by mixing dilute solution of sulphate of magnesia with a solution of carbonate of soda, charged with carbonic acid; they consisted of 29 base, 30 acid, and 41 water.—*Annals of Philosophy*, i. p. 254.

6. *On Compounds of Sulphur with Cyanogen*, &c.—M. Berzelius, in pursuance of his researches on the compounds of cyanogen, (p. 208,) has lately examined the sulphuretted compounds of cyanogen, and added much to our knowledge of them. He concludes that the substance, as prepared by M. Grotthus or M. Vogel, (*i. e.*, by fusing sulphur with ferro prussiate of potassa, dissolving, filtering, and drying,) is a sulphocyanuret of potassium; and though he has not been able to separate the sulpho-cyanogen or sulphuret of cyanogen from the base, so as to have it in a separate state, yet he deduces its composition from experiments, as being 1 atom cyanogen, and 2 atoms of sulphur, or

Carbon	20.63	2 atoms	150.66
Azote	24.28	1 atom	177.26
Sulphur	55.09	2 atoms	402.32
	100.		730.24

The sulpho-cyanuret of potassium is composed of

Potassium	40.15	1 atom		979.83
Azote	14.53	2 atoms	354.52	1460.48
Carbon	12.35	4 atoms	301.32	
Sulphur	32.97	4 atoms	804.64	
	100.			2440.31

The sulphuretted hydro-cyanic acid is composed of

Hydrogen	1.68	2 atoms	12.44
Nitrogen	23.85	1 atom	177.26
Carbon	20.30	2 atoms	150.66
Sulphur	54.17	2 atoms	402.32
	100.		772.68

In considering the nature of these substances, M. Berzelius seems inclined to admit that view of them and their nature which is analogous to the chlorine theory, as also this theory itself, and goes a considerable way towards answering some of those objections which have been raised at different times to it.

On substituting selenium for sulphur in these and analogous experiments, results which might have been expected from the analogy of the two bodies took place; on heating it with the ferro-prussiate of potassa, a selenio-cyanuret of potassa was formed perfectly analogous to the sulpho-cyanuret.—*Ann. de Chim.* xvi. 23.

7. *Sub-sulphate of Alumina and Potassa.*—This salt has lately been examined, and its constitution made out by M. Anatole Riffault. It was prepared by adding potassa to a boiling solution of alum until nearly saturated; a precipitate fell, which, when well washed and dried at a heat below 212°, was the substance in question. On analysis, it gave sulphuric acid 36.187; alumina 35.165; potassa 10.824; water 17.824; which accords with

1 atom sulphate of potassa	109.10	19.654
3 atoms subsulphate of alumina	344.84	52.116
9 atoms water	101.18	18.230

Such being the nature of this salt, and such its accordance with the atomic theory, attempts were made to produce a similar compound of sulphuric acid and alumine with ammonia. This was obtained by pouring solution of ammonia into a boiling solution of ammoniacal alum. The precipitate, when well washed and dried, was analyzed, and found to consist of sulphuric acid 38.248, and of alumina 37.851 *per cent.*, which accords very nearly with the full or theoretical number.

4 atoms sulphuric acid	200.46	38.724
9 atoms alumina	194.49	37.572
1 atom ammonia	21.55	4.164
9 atoms water	101.18	19 540
	517.69	100

Ann. de Chimie. xvi. p. 355.

8. *Analysis of Verdigris.*—Mr. Phillips has lately analyzed the acetate of copper by the following process. A given weight

was decomposed by being boiled with excess of hydrate of lime, which threw down all the oxide of copper, and formed an acetate of lime; this was filtered, and carbonic acid passed through the solution to separate any free lime dissolved in it; it was then heated, that the excess of carbonic acid and carbonate of lime might separate, and was then neutral acetate of lime, containing the acetic acid of the verdigris. The lime was then thrown down by carbonate of soda, and the carbonate of lime ascertained, and the quantity of acetic acid in the verdigris deduced from its quantity; 63 of carbonate lime being considered as equivalent to 63.96 of acetic acid. The quantity of oxide of copper in the salt was ascertained by precipitating the salt with potassa, and drying the precipitate. The water was deduced from the loss of weight. In this way 100 of acetate of copper appeared to be composed of

Acetic acid	49.2
Peroxide of copper . . .	39.2
Water	11.6

so that admitting peroxide of copper to be a compound of two atoms oxygen 20, and one atom copper 80, the atomic constitution of verdigris will be

	By Theory.	By Experiment.
2 atoms of acetic acid	127.92	128.84
1 atom of peroxide of copper	100.	102.65
3 atoms of water . .	33.96	30.39
	261.88	261.88

Mr. Phillips, in addition to Dr. Thomson's arguments for concluding the sulphate of copper to be a bi-salt, states, that if finely-divided carbonate of lime be added to a solution of it, an effervescence takes place, and an insoluble sulphate of copper falls. The same effect is produced by the soluble sulphate, nitrate, and acetate of copper, and hence Mr. P. is inclined to conclude that these also are bi-salts.—*Annals of Philosophy*, i. 417.

9. *Analysis of Gun-Powder.*—The usual mode of analyzing gun-powder is to take a given weight of it well dried, to dissolve out the nitre, to evaporate the solution and get the weight of nitre, and to get the weight of charcoal and sulphur by drying them together and weighing them. Another portion of gun-powder is then taken and heated with as much potassa and a little water, which dissolves out the nitre and sulphur, leaving the charcoal, which, being washed and dried, is weighed. The sulphur is estimated by the weight wanted to complete the sum of the weight of the nitre and charcoal.

Another mode has been adopted in the laboratory, of the direction of powders, which is shortly as follows. A certain portion of the powder is first dried and the loss of water ascertained; the nitre is then obtained by washing the powder, evaporating the solution, and fusing the nitre. To obtain the sulphur, 5 grains of powder, 5 of sub-carbonate of potassa, 5 of nitre, and 20 of common salt, all of them free from sulphuric acid, are mixed very intimately, and placed on the fire in a platinum vessel. The sulphur burns slowly, and the mass becomes white. It is then dissolved, and the solution being saturated with nitric or muriatic acid, the sulphuric acid is precipitated by muriate of baryta, the quantity of sulphate of baryta ascertained, and the sulphur estimated from its weight. The sulphate of baryta may either be collected on a filter and weighed, or it may be estimated by using a solution of muriate of baryta of known strength, adding it carefully until precipitation ceases; the quantity of solution used, gives by a direct proportion the quantity of sulphur in the gun-powder. The charcoal is estimated from the loss of weight.

Potassa, with some precaution, may be used for the sub-carbonate, and a capsule, a matrass, or even a tube of glass, may be used instead of the platinum capsule.—*Annales de Chimie*, xvi. p. 437.

10. *On firing Gun-Powder by Electricity*, by M. Leuthwaite.

DEAR SIR,—I have been induced to try a few experiments on the firing of gun-powder by means of the electrical shock, with the view of ascertaining the effect produced by making various fluids part of the conducting chain for the discharge. This inflammation has generally been considered as difficult to be produced. Probably these observations may tend to lessen the difficulty, and illustrate, at the same time, the conducting power of fluids for electricity.

The jar made use of contained one square foot of coated surface, and when charged with sufficient intensity to raise the quadrant electrometer to 90°, always discharged itself spontaneously.

The glass tube used was six inches long, the bore $\frac{3}{16}$ of an inch in diameter. A cork was fitted into each end, through which a brass wire was introduced, and the capacity of the tube was filled with different fluids.

I first ascertained from the mean of several experiments, that the gun-powder could not be fired through water, when the quadrant electrometer stood at a less number of degrees than 60, at which it always fired.

I then filled the same tube with sulphuric ether, and found from the mean of several experiments, that it would not fire the powder at a less number of degrees than 60. But when the tube was filled with alcohol, it always fired the powder at 30°.

The tube was, lastly, filled alternately with sulphuric and muriatic acid, and a discharge made through them when the electrometer indicated 80° of intensity, at which the gunpowder would not take fire.

Should you consider any of the foregoing experiments of importance, I shall leave you to make what use of them you may think proper. I am, &c.

Rotherhithe, 24*th May*, 1821. J. Leuthwaite.

11. *Use of Chromate of Lead as a Dye.*—The following observations on this subject are by M. Berthier: "The chromate of lead applies very well on stuffs, and I have many times repeated the experiment. The following are some remarks I have made.

"With subacetate of lead and neutral chromate of potassa, only an orange colour is obtained, not very agreeable; but if the stuffs thus dyed be dipped in acetic acid, they almost immediately acquire a very fine and brilliant yellow lemon colour. On using the neutral acetate of lead in place of the subacetate, a fine gold colour is immediately obtained, with the chromate of potassa, but acetic acid cannot give it the yellow lemon colour.

"These colours are absolutely unalterable by soap and water when cold; at boiling temperatures they fade a little, without any change of tint, but vinegar restores their first brilliancy.

"Ammonia makes them of a red orange colour; acetic acid restores them to their primitive state. When chromate of lead is treated by ammonia, it may be made to pass through a great variety of shades from orange to the red of the finest minium. The ammonia in these cases dissolves chromic acid, and leaves a red chromate. The action of acid, either nitric or acetic, is to dissolve oxide of lead from this chromate, and leave the clear yellow compound.

"Stuffs dyed by the chromate of lead, have their colours immediately and completely destroyed by the subacetate of soda and by muriatic acid, even when cold."—*Annales des Mines*, vi. 137.

12. *Porcelain Glaze.*—Mr. Rose, of Coalport, Shropshire, has sent a communication to the Society of Arts, in which a new glaze for porcelain is described. The object was to produce a glaze containing no lead, so that colours afterwards laid on to, and burned into, it, should not be altered by that metal. The principal ingredient in it is feldspar, of a somewhat compact texture, and a pale flesh red-colour, which forms veins in a slaty rock, adjoining to the town of Welchpool, in Montgomeryshire. This material being freed from all adhering pieces of slate and quartz, is ground to a fine powder; and, being thus prepared, 27 parts are mixed with 18 of borax, 4 of

Lynn sand, 3 of nitre, 3 of soda, and 3 of Cornwall china clay. This mixture is melted into a frit, and is then ground to a fine powder, 3 parts of calcined borax being previously added.

Some specimens furnished by Mr. Rose, were placed in the hands of Mr. Muss and other artists, to be submitted to experiments. When placed in heats much higher than they would be subjected to in the fair course of enamelling, the glaze remained firm and perfectly uniform, without any specks or splits having been produced on its surface; the colours, even the pinks and chrome greens, coming out remarkably well upon it, and none of them chipping off, as is frequently the case with the colours of the French porcelain.—*Transactions of the Society of Arts*, 1820.

13. *On preventing the Ravages of Moths in woollen Cloth.*—The discovery of an easy and effectual method of preventing the destruction of woollen fabrics and furs by moths, has long been a subject of research, and it still stands, I believe, among the list of premiums in the promises of the Society of Arts. Although the process here in question is known to many individuals, it is not yet known to the public at large, and your Journal offers the means of diffusing it.

The discovery, although accidental, is due to the officers of Artillery at Woolwich, employed in the inspection of clothing returned from Spain. It was observed, that in casks where all other woollen substances were totally destroyed, those cloths that had been rendered water-proof by the common well known processes, remain untouched. Attention having thus been excited to this circumstance, other similar mixed packages were examined, and the results were found to be invariable.

This process has the advantage of being cheap, easy of application, and permanent; since the chemical change produced by it in the surface of the woollen fibre, is not liable to be affected by time. If, in the case of military stores, no other good result were to follow the use of the water-proof process, this would be a sufficient reason for its universal adoption. The effect of all the odorous bodies commonly used for this purpose is transitory, as they evaporate in the course of time; but the aluminous soap which becomes united to the animal fibre in the water-proof process, seems to disgust this destructive larva so as effectually to prevent it, like some dyes, from attempting to devour the wool or other animal hairs, which are its natural food.

There seems no reason why this process should not be adopted in furs for the same purpose; since great losses are occasionally sustained by their destruction. It might with equal ease be applied to them; and as it does not appear to produce any effect on the appearance of the woollen substances to which it is applied, it would probably cause no change in the brilliancy or beauty of those substances, so justly valued for their utility and beauty, and so difficult to preserve without the most watchful attention.

J. M.

14. *Decomposition of Blood.*—M. Vauquelin had occasion to observe the changes produced in five years, in the fluid obtained by washing coagulated bullocks' blood. It appears at first to have contained the serum, and a considerable portion of the colouring matter of the blood, and the results at the end of the time mentioned were:

1. A large quantity of carbonic acid.
2. A large quantity of sulphuretted hydrogen.
3. A large quantity of acetic acid.
4. Ammonia which saturated these acids.
5. An acid and very fetid volatile oil, saturating part of the ammonia. These substances did not exist previously in the blood.
6. It appears that the fixed fatty matter which was found, had existed in the blood previously, as a similar matter was found in recent blood.
7. That the albumen was almost entirely decomposed, slight traces only remaining, and so altered in its nature that it rather resembled glue than albumen.
8. That the colouring matter remained entirely unchanged.
9. That the blood did not appear to contain phosphorus.

M. Vauquelin remarks that the quantity of sulphur in blood is much larger than is generally imagined, amounting to two grammes (about 30 grains) in a litre ($2\frac{1}{8}$ pints). This sulphur had separated spontaneously from the fluid, and formed a ring just above its surface on the glass.—*Ann. de Chim.* xvi. p. 363.

15. *Diod griàfol.*—A liquor is brewed from the berries of the mountain ash, in North Wales, called diod griàfol, by only crushing and putting water to them. After standing for a fortnight it is fit for use, its flavour somewhat resembles perry.

16. *Formation of Alcohol, by fluoboric Gas.*—Some very interesting experiments are detailed in a short paper published in the *Annales de Chimie*, xvi. p. 72, on the action of fluoboric gas on alcohol. They are by M. Desfosses of Besançon. The gas was sent into a portion of alcohol, which became very ethereal in odour, and very acid, even so as to fume. The fluid was distilled, and then rectified, first from potassa and afterwards from chloride of calcium. The ether thus obtained, was entirely analogous to sulphuric ether. It burnt like it, and gave no acid fumes. The specific gravity was .75, being rather greater than that of pure ether, but it had not been washed so

as to separate the alcohol from it. When decomposed in a tube heated red, it gave much carburetted hydrogen; but the water through which the gas had passed, was not at all acid, and did not effect either lime-water or acetate of lead. Hence it appears, that the ether should be ranged with those produced by sulphuric, phosphoric, and arsenic acids.

To ascertain what the phenomena of this change were, alcohol was saturated with fluoboric gas; after some time it became turbid, and a black powder like charcoal was deposited. It was distilled in an apparatus which would collect the gas; none came over during the time that ether was passing, but at the end of the distillation a few bubbles were liberated, which, when well washed, troubled lime-water, and were inflammable, they were therefore mixtures of carburetted hydrogen and carbonic acid; and these gases from other experiments appear to be produced by the action of the gas on the alcohol, and not by free sulphuric acid. Though the evaporation was carried very far, no sweet oil of wine was formed. From hence it follows, says the author, that first, an ether analogous to sulphuric ether, may be obtained by the action of fluoboric gas on alcohol; and, secondly, that the etherification probably takes place in consequence of the affinity of fluoboric acid for water, that it produces no sweet oil of wine, and that the acid does not appear altered in its nature.—*Annales de Chimie*, xvi. p. 72.

17. *On the Ripening of Fruits.*—In consequence of a prize question set forth by the Academy of Sciences, for the year 1821, three papers were received on the ripening of fruits, their effect on the air, *&c.* Of these, one written by M. Berard of Montpellier, gained the prize, and it has since been published in the French Journals, *Annales de Chimie*, xvi. p. 152, 225. The memoir is long, and cannot well be abridged, but the author has himself given a summary at the end of his paper of which the following is a translation:

Fruit does not act like leaves on the air. The result of its action as well in light, as in darkness, is at every instant of its formation, a loss of carbon by the fruit, which combines with the oxygen of the air, and forms carbonic acid. This loss of carbon is essential to the ripening of the fruit, for when the fruit is placed in an atmosphere deprived of oxygen, this function becomes suspended, the ripening is stopped, and if the fruit remains attached to the tree, it dries up and dies.

A fruit which happens naturally to be enclosed in a shell may nevertheless ripen, because the membrane which forms the husk is permeable to the air. The communication between the external and internal air is so free that the two portions are always of uniform composition, so that when the air thus contained is analyzed, it is always found to be of the same composition as atmospheric air.

When fruits separated from the tree, but capable of completing their own ripening, are placed in media free from oxygen, they do not ripen: the power, however, is only suspended, and may be re-established by placing the fruit in an atmosphere capable of taking carbon from it. But if the fruit remain too long in the first situation, although it preserves the same external appearance nearly, it has entirely lost the power of ripening.

Hence it results, that most fruits and especially those that do not require to remain on the tree, may be preserved for some time, and the pleasure they afford us thus prolonged. The most simple process consists in placing at the bottom of a bottle, a paste formed of lime, sulphate of iron, and water, and afterwards to introduce the said fruit, it having been pulled a few days before it would have been ripe. These fruits are to be kept from the bottom of the bottle, and as much as possible from each other, and the bottle to be closed by a cork and cement. The fruits are thus placed in an atmosphere free from oxygen, and may be preserved for a longer or shorter time according to their nature; peaches, prunes, and apricots from twenty days to a month; pears, and apples for three months. If they are withdrawn after this time, and exposed to the air, they ripen extremely well; but if the times mentioned are much exceeded they undergo a particular alteration, and will not ripen at all.

Ripe fruit exposed to the air rots and decays. In this case it first changes the oxygen of the surrounding air into carbonic acid, and then liberates from itself a large quantity of the same acid gas. It appears that the presence of oxygen gas is necessary to the rotting or decay of fruits, when it is absent, a different change takes place.

When the fruit cannot ripen except on the tree, its ripening is not produced by a chemical change of the substances it contained whilst still green, but by the change of new substances furnished to it by the tree, and when it appears to lose the acid taste it had in its unripe state, it is because that taste is hidden by the large quantity of sugar it receives in ripening.

In the fruits which ripen off the tree, the quantity of sugar is also found considerably to increase; and in this case, it must be formed at the expense of the substances previously in the fruit. Gum and lignin are the only principles, the proportion of which diminish at the same time; it is therefore natural to conclude, that it is the portions of these substances which have disappeared, that have been converted into sugar: and as the lignin contains most carbon, it is natural to suppose it is from it the oxygen takes the carbon to form carbonic acid, that change so indispensable to ripening*.

Finally, the alteration the lignin suffers in the ripening, continues during the decay of the fruit. It becomes brown, and its decomposition occasions the formation of much carbonic acid. Sugar is also decomposed at this time; and it is to its disappearance, that the peculiar taste of decayed fruits is to be attributed. The sugar in its decomposition also gives rise, no doubt to the formation of carbonic acid.

18. *Crystallization of Sugar.*—M. H. Braconnot has pointed out the strong powers of crystallization in the sugar of barley, and has shewn a remarkable instance in which that highly-important arrangement of matter can take place without liquidity. This sugar, when newly prepared, is perfectly transparent, and of a very brittle and vitreous fracture, offering at this time no traces of crystallization; but when left to itself for some days, its surface becomes dull and crystalline, and the effect continues to increase until the whole of the sugar has crystallized. It has then lost some of its transparency, and is seen to consist of many rounded groups of needle-like crystals, which are most generally separated from each other by empty spaces. The sugar, thus crystallized, is much more brittle than before; its fracture presents a number of acicular diverging crystals, united in bundles terminated by the interstices, especially when this arrangement has been produced at temperatures below the common temperature. When held in the mouth, it does not take on a bright smooth surface, but becomes rough, and by care small crystals may be separated, which by the microscope appear to be flat tetrahedral crystals. It was at first supposed that this substance had attracted water from the atmosphere, and in that way been enabled to have such motion produced among its particles as to allow of crystallization; but when placed in a close vessel with chloride of lime, when it lost $\frac{1}{200}$ of its weight, still it crystallized as in the open air. It also crystallized when immersed in oil of turpentine.

Confectioners know, and have long feared, the effects of this crystallization; they considered it as a degradation of the sugar in its nature, and search continually for means to prevent it but apparently without success.—*Annales de Chimie*, xvi. p. 427.

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* M. Berard in a note says, it is difficult to suppose that in those fruits that ripen early on the tree, all the sugar should be sent into the fruit from the plant; it is more probable that the fruit acts on the air, and forms sugar like the other fruits, but not in sufficient quantity, and that therefore, it is necessary recourse should be had to the tree, to complete its ripening.

19. *Cathartine, the active Principle of Senna.*—MM. J. L. Lassaigne and H. Fenuelle, in examining senna, obtained from it a particular principle called by them cathartine. A decoction of the leaves was made, and, after being filtered, was precipitated by acetate of lead. The precipitate collected was diffused through water, and sulphuretted hydrogen passed through it. The liquor filtered was evaporated to dryness, and digested in alcohol, and the alcohol solution then evaporated to dryness. It contained acetate of potassa, which was separated by alcohol acidulated by sulphuric acid; then filtering to separate the sulphate of potassa insoluble in this fluid, precipitating the excess of sulphuric acid by acetate of lead, decomposing this latter salt by sulphuretted hydrogen, filtering again and evaporating to dryness, a substance was obtained, which was considered the purgative principle of senna.

This substance is uncrystallizable, of a reddish yellow colour, of a particular smell, a bitter and nauseous taste. It is soluble in alcohol and water in all proportions; insoluble in ether. Its extract becomes moist in the air. It purges in very small doses.—*Annales de Chimie*, xvi. p. 20.

20. *Piperin, or the active Principle of Pepper.*—Piperin is a new vegetable principle, extracted from black pepper, by M. Pelletier. To obtain it, black pepper was digested in alcohol repeatedly, and the solution evaporated, until a fatty resinous matter was left. This, on being washed in warm water, was left of a good green colour, and had a hot and burning taste; it dissolved readily in alcohol, and less readily in sulphuric ether; concentrtaed sulphuric acid gave it a fine scarlet colour. A solution of this substance in hot alcohol, being left for some days, deposited a number of small crystals. These were purified by repeated solution and crystallization in alcohol and ether, and from the mother liquors, fresh portions were obtained, which, on purification, were like the first. It is to be remarked, that the pepper taste they possessed when impure, gradually left them as they became more and more pure; so that the white crystals scarcely had any taste, while it seemed to accumulate in the fatty matter, as the crystalline portion was separated from it: and also, that the purer the crystals, the finer the tint produced in them by sulphuric acid. The fatty matter left, also reddened by sulphuric acid; but it is a question whether it would do so when pure.

The crystalline matter forms colourless four-sided prisms, with single inclined terminations; they have scarcely any taste. Boiling water dissolves a small portion; but it is insoluble in cold water; it is very soluble in alcohol, less so in ether; it is soluble in acetic acid, and crystallizes from it in feathery crystals. Weak sulphuric, nitric, and muriatic acids, do not dissolve or act on it; the strong acids decompose it. Strong sulphuric acid gives it a blood-red colour, which disappears on adding water; the substance does not seem altered if the acid has not remained long on it. Muriatic acid acts in the same way, producing a deep yellow colour. Nitric acid makes it greenish yellow, orange, and then red; the ultimate action of the acid produces oxalic acid, and yellow bitter principle. It melts at about 212°. Destructive distillation converts it into water, acetic acid, oil, and carburetted hydrogen, gas: no ammonia is formed. Oxide of copper converts it into carbonic acid and water. After comparing this substance with other vegetable principles, particularly with resins, M. Pelletier concludes by considering it a peculiar substance, and names it *Piperin*.

The fatty matter left, after extracting the piperin, is solid, at a temperature near 32°; but liquefies at a slight heat. It has an extremely bitter and acrid taste; it is very slightly volatile, and tends rather to decompose, than rise in vapour; that which passes over is not so piquant and acrid, as the undistilled part, but is more balsamic. It dissolves easily in alcohol and ether, and unites to fatty bodies; and, with the exception of its taste, does not differ from them. From the result of the distillation, it may be considered as composed of two oils: one volatile and balsamic; the other more fixed, and containing the acridness of the pepper.

Finally, M. Pelletier finds in pepper—1, Pipirin; 2, a very acrid concrete oil; 3, a volatile balsamic oil; 4, a gummy coloured matter; 5, an extractive principle; 6, malic and tartaric acids; 7, starch; 8, bassorine; 9, lignin; 10, earthy and alkaline salts. He concludes, also, there is no vegetable alkali in pepper; that the crystalline substance of pepper is a peculiar body; that pepper owes its taste to an oil but little volatile; and that a strong similarity exists between common pepper and cubebs, as illustrated by the analysis of M. Vanquelin, of the latter compared with the former.—*Annales de Chimie*, xvi. p.337.

21. *On Phosphorescence.*—The phenomena of phosphorescence, produced by exposure of bodies to light, have been very attentively observed lately by M. Heinrich, of Ratisbonne, who has made some new and interesting observations on them. The precautions taken by the observer were to remain, previous to the observation, thirty or forty minutes in a perfectly dark place; to expose the substances, the powers of which were to be observed for not more than ten seconds to the light of a

clear day; to keep them out of the rays of the sun, lest they should become heated; and to observe them in the same dark chamber to which he had previously retired. The observations were made in two different seasons, the summer and the winter. The following are the general results, but the account is very much compressed from M. Heinrich's paper.

Among natural bodies some are phosphorescent, some not phosphorescent. Among the most phosphorescent are some diamonds, but not all, though no apparent difference in their external appearances could be perceived; some remained luminous from five seconds to one hour. The different effects of the coloured rays were remarkable; a good diamond exposed in the blue rays acquired a durable phosphorescence, but did not become luminous at all in the red rays. All the fluor spars were highly phosphorescent, and also all the carbonates of lime.

M. Heinrich observes that the phosphorescence of calcareous combinations varies with the acid in combination. Thus the fluates were very phosphorescent, some of them remaining luminous for one hour. The carbonates follow; they are distinguished by the clear and white light they emit, which is such in some specimens as to enable a person to read by it, but it does not continue above thirty or forty-five seconds. The sulphates shine for a short time, but very faintly; the phosphates are still less favourable for the phenomena of phosphorescence by irradiation.

The calcareous combinations are succeeded by heavy spar or sulphate, and carbonate of baryta. Pure siliceous, aluminous and magnesian earths are not phosphorescent, though many of their native combinations are feebly so.

With regard to saline minerals, M. Heinrich considers their phosphorescent powers to be determined by the acid and base in combination. With the exception of amber and the diamond, no inflammable fossil becomes phosphorescent by irradiation. None of the metals are phosphorescent; metallic salts are moderately, and metallic oxides feebly, phosphorescent.

Vegetable substances are but bad phosphori. The wood of hot countries is better than that of our climate; the white hazel-tree shines brightly—an old sugar cane, dates, and the inner part of the cocoa nut, become finely phosphorescent. Cotton is very bad; dried plants are in general very feebly luminous; vegetable substances bleached are infinitely superior to the same substance not bleached; this is particularly observable in linen, paper, *&c.* Animal substances, containing carbonate of lime, as egg-shells, shells, corals, *&c.*, are more phosphorescent than those containing phosphate of lime.

After the facts, of which the above are the results, follow various interesting observations: thus it is remarked that the duration of the light differs very much; the diamond and fluor shine for above an hour, no other fossil for more than a minute. Vividness and duration are in no relation to each other. With the exception of the diamond, the light of fossils is always white, whether they be illuminated by coloured rays or not. Sun-light is more effectual in producing the phenomena than day-light, but too long an exposure is disadvantageous. White bodies act more powerfully than coloured, and light coloured more than dark coloured. Bodies that are shining continue to shine when immersed in water. Difference of temperature has but little effect; ice is phosphorescent, at the same time it is observed, that heat augments the intensity and diminishes the duration of the phosphorescence; cold has a contrary effect. Pure water and transparent fluids do not shine. The light appears to penetrate the substances considerably; for when shining, if a grove be cut a line deep in the substance, it will be as luminous at the bottom of it as at the surface; finally, polishing injures, and in some cases destroys, the phosphorescent power.

M. Heinrich then speaks of artificial phosphori, and describes the process for preparing the Bolognian compound, and those of Canton, Baldwin, *&c.* Among animal and vegetable substances, many not luminous at first, became so by being cooled or heated; some of these are the flesh of birds; dried tendons, burnt bones and horns, yolk of eggs, toasted cheese, roasted coffee, chestnuts, pease, *&c.*

M. Heinrich considers these phenomena of phosphorescence to be occasioned by the mere restitution or emission of the light absorbed by the phosphori during exposure to external sources. Among those which are most difficult to explain are the preservation of the light, by enveloping the luminous body perfectly; and its increased emission by the application of heat. If a diamond, the fluor spar of Siberia, or chlorophane, be exposed to light for some minutes, and be then covered with black wax, ink, or any substance, which will perfectly exclude air and light, on removing the envelope, after some days, the body will still be found emitting light. This fact was known to Kircher and Beccaria. The second fact is the following: When fluor spar which has been exposed to light has ceased to become luminous, it may be made to emit light by merely warming it with the breath or the hand. This effect may be obtained many times successively after only one irradiation, especially if, at each time, the warmth be a little increased; at last, warming ceases to produce the effect, but the simple exposure of the spar to the sun enables it anew to present the same appearances as before.—*Bib. Univ.* xv. p. 247.

22. *To the* EDITOR *of the Journal of Science.*

SIR,

IN page xiii of the Introduction to the Dictionary of Chemistry lately published, I have alluded to Dr. Henry in terms which have occasioned a private correspondence between that gentlemen and me, the result of which we are desirous of making public in your Journal.

In the beginning of August 1816, I transmitted to him an Essay on Alkalimetry and Acidimetry, accompanied by a letter, in which I begged him to favour me with his opinion of its merits, cautioning him, meanwhile, not to communicate its contents to any person. In the 8th edition of his Elements, which appeared in 1818, he published a plan of alkalimetry and acidimetry modified from that described in my Essay*. This struck me at the time as an unwarranted use of my communication; and declining to correspond with him on the subject, I resolved to seize the first favourable opportunity to reclaim my rights. Under this feeling I wrote the paragraph in the Introduction to the Dictionary.

Dr. Henry thus writes me on the 12th of April 1821, " I assure you that I had not at the time of publishing my book, nor can I now recall, the remembrance of any injunction of secrecy, respecting your alkalimeter; I conceived I had so expressed myself at page 512, vol. ii. of my Elements, as unequivocally to give to you the credit of inventing an instrument on the principle of directly, and without calculation, indicating the *per centage* of alkali in any specimen; and that I pretend to nothing more than the modification of your method which is described in my book."

Under these circumstances, I am satisfied that Dr. Henry had no intention to appropriate to himself the credit of my invention; but I sincerely regret that, before promulgating the modification of my method, he had not consulted me on the subject. This would have prevented all chance of misunderstanding between me and Dr. Henry, whose accomplishments as a gentleman and a chemist, I have been accustomed to admire. The readers of the Dictionary will perceive under the articles CALCULI, COAL-GAS, GAS, SALT, &c., that I have not suffered temper to influence my judgment, but have done merited honour to the Doctor's researches on every scientific occasion.

I have the honour to be, Sir,
Your most obedient servant,
Glasgow, April 15, 1821. ANDREW URE.

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* " It has been very properly objected to it [the alkalimeter of Descroisilles] by Dr. Ure, of Glasgow, (in an Essay on Alkalimetry, which he was so good, about two years ago, as to communicate to me in manuscript, and which I believe he has not yet published,) that these degrees, being entirely arbitrary, do not denote the value of alkalis in language universally intelligible; and he has proposed an instrument which shall at once, and without calculation, declare the true proportion of alkali in 100 parts of any specimen. The principal deviation in the following rules from the method of Dr. Ure, is," &c. &c.

23. *Singular Property of Boracic Acid.*—I mentioned in the 6th vol. of this *Journal*, p. 152 (1819), the property possessed by boracic acid in all states of dilution, of reddening turmeric paper in the manner of an alkali. Since then the attention of M. Desfosses has been drawn to the action of boracic acid on this colouring matter (*Annales de Chimie*, xvi. p. 75.), apparently without a knowledge of the previous remark; and he has shewn that a mixture of boracic, with other acids, reddens turmeric very deeply, and that turmeric, when acted on by this mixture of acids, has its nature altered, for it approaches somewhat to turnsole, and is rendered blue by alkalies.

There is something so curious in this action of boracic acid, on turmeric, that I am tempted to offer a few more results on the subject.

Turmeric paper dipped into a solution of pure boracic acid very speedily receives a slight tint of brownish red, which, when the paper is dry, is very marked, and resembles that produced by a weak alkali. In this state the properties of the colouring matter are entirely different to what they were before: sulphuric, nitric, muriatic, and phosphoric acids, even when very dilute, produce a bright red colour on this paper, and a strong solution of oxalic acid also reddens it. Alkalies on the contrary make it blue, gradually passing to shades of purplish blue, yellowish red, *&c.* As long as the acids or alkalies remain on the paper, if not so strong as to destroy the colouring matter, the new colour remains, but a slight washing removes them, and then the boracic acid tint returns, and the paper has its first peculiar properties. When altered by muriatic acid, or ammonia, the mere volatilization restores the paper to its first state; with the ammonia the restoration is very ready and perfect; with the acid, it is longer and not so complete. If the paper reddened by boracic acid be heated, the yellow of the turmeric is almost restored, and then it takes from acids a weaker red tinge, and from alkalies a more purplish colour than before.

Turmeric, thus altered by boracic acid, is readily restored to its original state by washing; altered turmeric paper when put in water for two or three hours resumes its original properties, and acts as at first in testing the alkalies.

When the altered paper is placed in sun-light a few days, the colour is soon destroyed as with turmeric alone, and then neither acid nor alkali will affect it.

When turmeric paper is dipped into neutral or slightly alkaline borate of ammonia, it soon becomes of the red tint produced by boracic acid, and is, in every respect, as if altered by boracic acid alone; when this paper is made blue by ammonia, the ammonia easily washes out, and the blue tint disappears, and afterwards the boracic acid or borates will wash out and leave the paper as at first.

Borax itself at first reddens turmeric paper because of the excess of alkali, but as the colouring matter becomes altered by the presence of the boracic acid, the tint becomes of a dirty bluish colour, and then the paper is changed by acids or alkalies, just as if it had been altered by boracic acid.

Hence it is probable that the neutral borates have the same power as the boracic acid, of altering the colouring matter of turmeric, for it is not probable there should be an actual separation of the elements of the salts by it, especially as they both wash out from it and leave it unaltered.

Hence also both acid and alkaline borates redden turmeric.

M. Desfosses, proposes this effect of boracic acid as a test for its presence; for a very small quantity of it mixed with other acid has the power of reddening turmeric paper in consequence of these changes.

M. F.

III. NATURAL HISTORY.

GEOLOGY, MEDICINE, &c.

1. *Further Remarks on the Resemblance between certain Varieties of Granite and of Trap.*—J. MAC CULLOCH.

IN a former number of this *Journal* (Vol. X. p. 29.), I gave a detailed account of some interesting facts occurring in Aberdeenshire, respecting the resemblance of certain portions of the granite of that country to some of the members of the trap family. It was there shown, that, in this district, specimens could be procured from the fundamental granite, and connected with the most common varieties by transition, resembling many of the latest greenstones, and even the basalt of most recent origin which is superincumbent on the latest stratified rocks. The series of specimens formed from these places, is not to be distinguished from a common series of basalt and greenstone; but the interesting conclusions to which this fact gives rise, respecting the similarity of origin in these two families, so far removed in position, need not be repeated, as they were sufficiently pointed out in the paper to which I have alluded.

As the instances which I there quoted may, however, seem to require confirmation, particularly to those geologists who are unwilling to abandon the notions in which they have been educated, it will not be useless to point out another set of similar facts, equally open to investigation, and equally confirming the views formerly held out. Setting aside this minor consideration, it is always useful to accumulate examples of any geological fact; particularly of such as, from their novelty, or from their disagreement with former observations, are often, for a considerable time, received with doubt or incredulity. To multiply the places of access to such appearances is also useful; and I can only regret that I have not here to refer to a country more accessible, instead of being, as it is, more remote than Aberdeenshire.

Granite of various characters occurs in different parts of the Shetland islands; where it displays, in a degree of profusion not to be equalled through the whole of Scotland, all these phenomena attending veins, and accompanying its contact with the stratified rocks, which are, deservedly, objects of so much attention to geologists, and which serve to throw so much light on the nature and origin of this substance. But the most entire and extensive tract is found in North Maven, extending over a space which it would be useless to describe in words; as, without a map, no definite idea could be conveyed of it. There are, at least, two very distinct varieties in this district; and, it is not difficult to discover that they are of different eras; since veins of the one variety are found to penetrate into the other, whereas the reverse never takes place.

It is in one of these that the varieties, analogous to those of Aberdeenshire, formerly described, are found; and they present a similar series of graduating specimens; the whole being evidently inferior to gneiss and the other primary strata of that district, and, in many places, graduating into undisputed varieties of the most ordinary granite. To detail the aspects of these specimens, would be merely to repeat that which was said in the former communication on this subject. I shall therefore merely add, that from the ordinary syenitic granite, consisting of hornblende, quartz, and feldspar, with or without mica, a regular series may be traced, passing through numerous modifications of greenstone, not differing from those of the trap family, down to a perfect basalt.

For the information of those who may be inclined to visit the ground in question, I may add that the most convenient situations for examining these appearances in detail, are in the neighbourhood of Hillswick.

2. *On the Deposition of Carbonate of Lime in Wood.*—It is well known that siliceous earth is deposited in many vegetables, particularly in the grasses, in the bark of the *Calamus Rotang*, and in that of *Equisetum hyemale*. The deposit known by the name of *Tabasheer*, is a particularly conspicuous example of this nature. The deposition of carbonate of lime is a more rare occurrence; yet it is found in many pears, and is very re-

markable in the bark or on the surface of *chara vulgaris.* Having accidentally observed one instance of this nature in a very unexpected situation, I thought it deserving of record as adding another illustration of a remarkable fact in vegetable physiology.

A few years ago, when some uneasiness was produced by the rapid consumption of oak in the navy, commissions were sent to various places to procure such woods as appeared to be adapted for ship-building. Among others; many specimens were brought from Sierra Leone in Africa; and, from these, the singular wood under review was selected. Unfortunately, no description of these trees was furnished; so that it is impossible to conjecture to what genus the specimen in question belongs, or whether indeed it belongs to any known genus.

The size of the timber, which is probably still lying in Deptford yard, proves, at least, that it is a large tree. The colour of the wood is that of mahogany, which it much resembles on a general view; being, at the same time, equally hard. But the longitudinal split shows a larger fibrous structure, and, in the transverse smooth section, the grain is coarser, from the large size of the vessels which form the interesting part of this wood.

These vessels, or tubes, are so numerous that they amount to 1,600 in the square inch. Their form is very irregular; seldom round, occasionally oval, but more commonly of a long irregular shape. Sometimes also, two or more ovals are connected by a narrow line. These vacuities in the wood are filled with a yellow carbonate of lime, which bears slight marks of irregular crystallization. But they are not always entirely filled; the wider ones being perforated by a circular tube running through them, and surrounded by the calcareous matter. These orifices are of such a size as just to admit the point of a human hair; it frequently happens that two or more are contained in one of the deposits of the carbonate.

I need scarcely add that the application of an acid excites an active effervescence over the whole section of the wood.

3. *Breaking out of a Spring.*—A remarkable phenomenon occurred at Bishop Monckton, near Ripon, on April 18th, on the estate belonging to Mr. Sharnock. About two o'clock in the afternoon the attention of a person in that gentleman's service was attracted by a rumbling noise which apparently proceeded from the stack yard, distant thirty yards from the house. He supposed it to proceed from children throwing stones against the doors and wall; but on looking up the avenue, formed by a row of stacks, and leading to the house, he observed a small portion of the ground in motion, which, after continuing in a considerable state of agitation for some minutes, suddenly presented an opening of about a foot square, whence issued a great

body of water. Returning with violence it soon enlarged the cavity, and in its progress carried down with it a portion of the surrounding earth several feet in extent, which was buried in the abyss below; the water continued to ebb and flow more or less at intervals during the day. Mr. Charnock plumbed this subterranean pit in the evening, and found it fifty-eight feet in depth; the water has now subsided and remains settled within two yards of the top.—*Gentleman's Magazine*, May, 461.

4. *New Volcano.*—A new volcano has burst out in the highest summit of a ridge of mountains near Leiria, in Portugal. This extraordinary phenomenon occurred at the period of the high rise of the Douro, mentioned in most of the Journals. The volcano was in full action when the latest accounts came away, but had happily taken a direction which threatened to do little damage. The country is steril, and is that through which Wellington passed in pursuit of Massena.—*Gentleman's Magazine*, April.

5. *Hartshorn, its use in Intoxication.*—Dr. Porter, a German physician, states that he has found the spirit of hartshorn (in the dose of a small tea-spoonful in a glass of water,) to counteract the inebriating effects of fermented liquors and spirits.

6. *Scarlet Fever.*—It is announced in the *Journal de Medicina Pratique*, of Berlin, that the Belladonna is a preservative against this fever. The fact was first discovered at Leipsic, but it has lately been confirmed by several experiments.

7. *Iodine, on its application as a Medicine.*—An abstract was given at page 191, vol. x. of this *Journal*, from a paper, by Dr. Coindet, of Switzerland, on the application of iodine to the dissipation of the goitre. In consequence of the importance of any effectual remedy for this disease, in a country where it is so frequent, much attention has been drawn towards Dr. Coindet's discovery, and considerable opposition made to it. It happens also that from the number of cases in which it has been applied, much information, with regard to the general medicinal effects of this substance, has been obtained. These, with other reasons, have induced Dr. Coindet to publish a second paper on the subject, which, as it contains some very interesting matter necessary to be known before the publication of the remedy can be said to be completed, we are induced to abstract at this time, though from the rarity of the disease in this country, it has not that high interest here it possesses in that part of the world.

After having dwelt upon the necessity in every case of using prudence in the administration of a powerful medicine, especially when that medicine is new, and its action but little understood; Dr. Coindet mentions the circumstance that at Geneva alone 140 ounces of iodine have been sold since he first made known its use in this disease; consequently, that above 1,000 persons have used it; and he remarks that fewer accidents have happened in the application of this quantity than happens in a similar application of almost any powerful medicine.

As the iodine in different states will act differently as a medicine, Dr. Coindet states, that of all the preparations he prefers the ioduretted hydriodate of potassa. This is prepared by dissolving thirty-six grains of the hydriodate, and ten grains of iodine in one ounce of distilled water; from six to ten drops in half a glass of water, sweetened, is given three times a day, diminishing or increasing the dose according to the effects.

Dr. Coindet prepares the hydriodate of potassa by saturating potassa with hydriodic acid. The acid he prepares previously by passing sulphuretted hydrogen gas through water holding iodine in suspension, or through a solution of iodine in alcohol. The sulphur is then filtered out, and the liquor heated to drive off the free sulphuretted hydrogen. A much simpler mode of preparing the hydriodate would be to saturate a strong solution of potassa with iodine, evaporate to dryness, and fuse the salt out of contact with air in a covered platinum crucible or glass flask, until the portion of iodate formed is decomposed and converted into iodide; the whole is then iodide of potassium, and only requires to be dissolved in water to form the hydriodate of potassa.

Whilst attentively observing the action of this substance on the animal economy, it soon appeared that if given in excess, it seemed to saturate the body, and then produced particular symptoms, which Dr. Coindet calls iodic. This never happens before an effect has taken place on the goitre; and, as the farther addition and action of iodine, beyond the dissipation of the mass is injurious, a stop is immediately put to its administration when these effects appear. After eight or ten days its use is resumed, and continued until the symptoms are again observed, when it is discontinued and again resumed after an interval of time, which is to be more or less according to the state of the patient, and the effect of the medicine on him.

The iodic symptoms when strong are as follows: accelerated pulse, palpitation, frequent dry cough, want of sleep, rapid loss of flesh and strength; with some, there is produced only a swelling of the legs, or tremblings, or a painful hardness of the goitre, sometimes diminished breasts, continued increase of appetite, and in all that Dr. Coindet had seen a very rapid diminution and disappearance of the goitre.

At those times Dr. Coindet forbade iodine and prescribed milk, especially that of asses, warm baths, valerian, kino, carbonate of ammonia, preparations of opium, and other antispasmodics. In painful hardness of the goitre; leeches, and emollient fomentations.

The rapid disappearance of the goitre, which accompanies these symptoms, shews them to be occasioned by an excess of iodine: from eight to ten weeks is considered the mean time of proper treatment.

The iodine should not be administered indiscriminately in all cases of goitre: some are inflammatory, and some are accompanied by a bilious disposition of the body; in these cases, leeches should be applied on the goitre, and medicines administered as the case requires, before the iodine be given. If similar symptoms arise during the application of iodine, then those indications should be attended to, and proper medicines given with the iodine.

Iodine should never be employed in cases where the patient is of a gross disposition, or tending to menorrhagia, or in cases where diseases of the breast threaten to, or have commenced, or in slow fevers. It should also be refused to persons who are nervous, delicate, and of a feeble constitution.

Dr. Coindet then states his reasons for believing that iodine may be usefully employed in cases of amenorrhœa, in chronic diseases of the uterus, of indolent tumours of the lymphatic glands of the breast, cases of scrophula without fever, and where the enlarged glands of the neck are indolent; and concludes by expressing a strong wish that no person will resort to this remedy without the advice and observation of a physician.—*Bib. Univ.* xvi. p. 140.

8. *Medical and Physiological Prize Questions.*—1. The Royal Academy of Sciences, at Paris, have proposed the following prize question for 1823: "To determine, by precise experiments, the causes, either chemical or physiological, of animal heat." It is particularly required that the heat emitted by a healthy animal in a given time be ascertained, as well also as the quantity of carbonic acid produced in respiration, and that the heat thus produced, be compared with that occasioned by the formation of as much carbonic acid from the combustion of carbon. The prize will be a gold medal of 3,000 francs value. The memoirs are to be sent in before July 1, 1823.

2. The following prize subject has been announced by the Royal Academy of Sciences, at Paris, for competition during the years 1821 and 1822: "To trace the gradual developement of the aquatic Triton, or Salamander, through its different degrees from the egg to the perfect animal, and to describe the internal changes which it experiences, but principally in regard to osteogany and the distribution of the vessels." The prize is

a gold medal of 300 francs value; to be adjudged in March, 1822. The essays to be sent in by January 1, 1822.

3. The *Société Medicale d'Emulation* proposes the following prize question: the memoirs, written in French or Latin, are to be sent before August 31, 1822, to the *Secrétaire-générale*, at Paris. The value of the prize is 500 francs.

"What are the disposition and structure of the system of organs called the nervous ganglions of the organic life, sympathetic nerve, great intercostal, trisplanchnique, *&c.*?"

"What are the functions of this system of organs?"

"And, as far as is known, what are the diseases in which it is essentially affected?"

4. The Helvetic Society of Natural Sciences have proposed the following prize question for the years 1822 and 1823: To collect exact and well-observed facts, on the increase and diminution of the glaciers in the different parts of the Alps, on the deterioration or amelioration of their pasturages, and on the former and present state of the forests." It will be sufficient if the authors treat only of a determined part of the Alps. Memoirs to be given in before the 1st of January 1823. Prize 300 livres.

5. The Society of Sciences and Arts of Utrecht has announced the following question for competition: "Are there characteristic signs sufficient to distinguish always with certainty, the true cancers from other maladies which resemble it? If so, what are these signs? Ought this malady to be considered as the effect of an indisposition of the whole body, or as only local? If it is to be considered as an indisposition of the whole body, can external remedies, whether amputation, or the remedy applied by the religious of the convent of Rus, or the corrosive remedies, especially arsenic, contribute to the cure or the alleviation of the malady? or ought they to be considered as all equally hurtful? When the malady has not yet the characteristic signs of true cancer, but when there is reason to fear it may become so, and when it may as yet be considered as a local evil, what external remedies may then be applied with sound hope of success? and what are those which should be considered as hurtful?"

6. Another question, by the same body, is as follows: "Can we, by surveying any particular part of the body of an animal that we have not had an opportunity of observing in life, conclude, with certainty, what use it made of that part; so that we may look on this principle of final causes, not only as an useful principle, but as always a sure guide in the natural history of the animal kingdom?"

IV. GENERAL LITERATURE.

1. *Origin of Vegetables.*—Turnips and carrots are thought indigenous roots of France; our cauliflowers came from Cyprus; our artichokes from Sicily; lettuce from Cos, a name corrupted into *Gause*; shallots, or eschallots, from Ascalon; the cherry and filbert are from Pontus; the citron from Media; the chestnut from Castana, in Asia Minor; the peach and the walnut from Persia; the plum from Syria; the pomegranate from Cyprus; the quince from Sidon; the olive and fig from Greece, as are the best apples and pears, though also found wild in France, and even here; the apricot is from Armenia.—*New Monthly Magazine*, iii. p. 235.

2. *New Longitude Act.*—By this act the 58th of the late king is amended. The rewards are, 5000*l.* to any subject of Great Britain who shall reach the longitude of 130° from Greenwich within the arctic circle; 10,000*l.* (further) for the north-west passage into the Pacific; 1,000*l.* for 83° of north latitude; and a like sum for 85°, 87°, 88°, and 89° respectively. It is assumed in the preamble, that no ship has gone beyond 81° of north latitude, nor 113° of west longitude.

3. *Roman Mint.*—A considerable quantity of clay moulds, or matrices, for the coining of Roman money, have been lately turned up at Lingwell-Yate, near Wakefield. Thoresby, in his *Ducatus*, mentions a quantity of similar moulds, found at the same place in 1697. Several crucibles, for melting the metal, were found at the same time; and in some of the moulds, there are coins yet remaining. Specimens have been sent by a gentleman at Wakefield, to the Society of Antiquaries, and to the British Museum, in hopes of their decision whether this place was the resort of coiners, or the real mint belonging to the Roman station in its immediate vicinity.—*New Monthly Magazine.*

4. *Ancient Roman Altar.*—A Roman altar was dug up in April, by Mr. S. Faulkner, gardener, in a place called Darvell's field, situate between the Tarvin and Whitchurch roads, in Boughton, near Chester. It is formed of red granite, and is in excellent preservation: its height is nearly four feet; its two fronts, on each of which is the same inscription, are eighteen inches across; and the two sides, quite plain, are about twelve inches each. On the top is a kind of shallow basin, supported by two volutes. The pedestal is a square piece of red sandstone, about twenty inches wide, and six thick, and was found at a small distance from the altar. The inscription is, "*Nymphis et Fontibus Legione Vicesima valente victrici;*" thus Englished: "The twentieth Legion, the power-

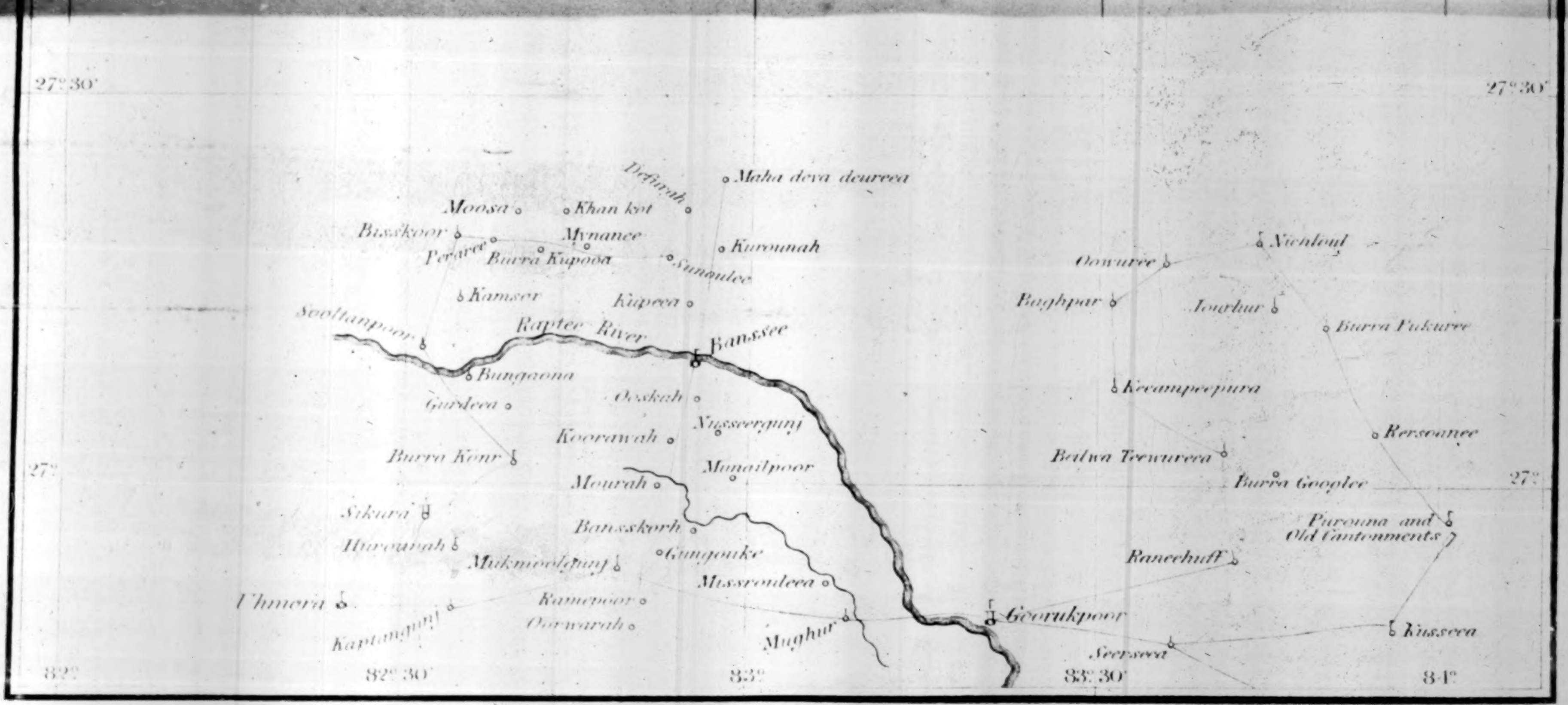
ful, the victorious, to the Nymphs and Fountains." There is no particular spring now known very near the spot, where the altar was found; but there are some within five minutes' walk of the place, and the district abounds with them. It is supposed this curious piece of antiquity was thrown down, and buried, at the time the Romans took their last leave of Britain, which was about the year 448 of the Christian era.—*New Monthly Magazine.*

5. *Druidical Sepulchre.*—Ten sepulchral urns were lately found about a foot below the surface on the grounds of Llys D'unfarm, the property of Joseph Huddart, Esq., near the Roman military communication between the tumulus at Llocheddier and that of Dolbermaon, in Caernarvonshire. The urns occupied a circular space about five yards in diameter, which seemed to have been surrounded by a stone wall. They lay in a straight line, and were filled with bones and ashes; the first containing a small piece of copper. Each urn was protected by four upright stones in a rectangular form, with a flat stone on the top, and a few handfuls of pure gravel underneath. They crumbled into ashes when the ploughman attempted to remove them, and not a fragment above the size of a square inch could be found a few days after the discovery. From there being several druidical remains in the neighbourhood, it is supposed to have been a place of sepulchre consecrated by the Druids. A great part of the sepulchre still remains untouched.—*Monthly Mag. May* 1821.

6. *Literary Notices.*—i. The first volume of Mr. A. P. Thomson's *Lectures on Botany* is almost ready for publication. It will contain the descriptive anatomy and physiology of those organs which are necessary for the growth and preservation of the plant as an individual; and will be illustrated by more than one hundred wood-cuts and ten copper-plates. It is intended to form the first part of a complete system of Elementary Botany.

ii. Next month will be published, *A Treatise of the Principles of Bridges by Suspension*, with reference to the *Catenary*, and exemplified by the Cable Bridge now in progress over the Strait of Menai. In it the properties of the catenary will be fully investigated, and those of arches and piers will be derived from the motion of a projectile. It will contain practical tables, a table of the dimensions of a catenary, and tables o the principal chain, rope, stone, wood, and iron bridges; with the dimensions of them erected in different countries.

iii. Mr. Gideon Mantell, F. L. S. is about publishing in one volume, royal quarto, (illustrated by numerous engravings), the *Fossils of the South Downs; or, Outlines of the Geology of the South-eastern Division of Sussex.*



James Basire & Son, sculp

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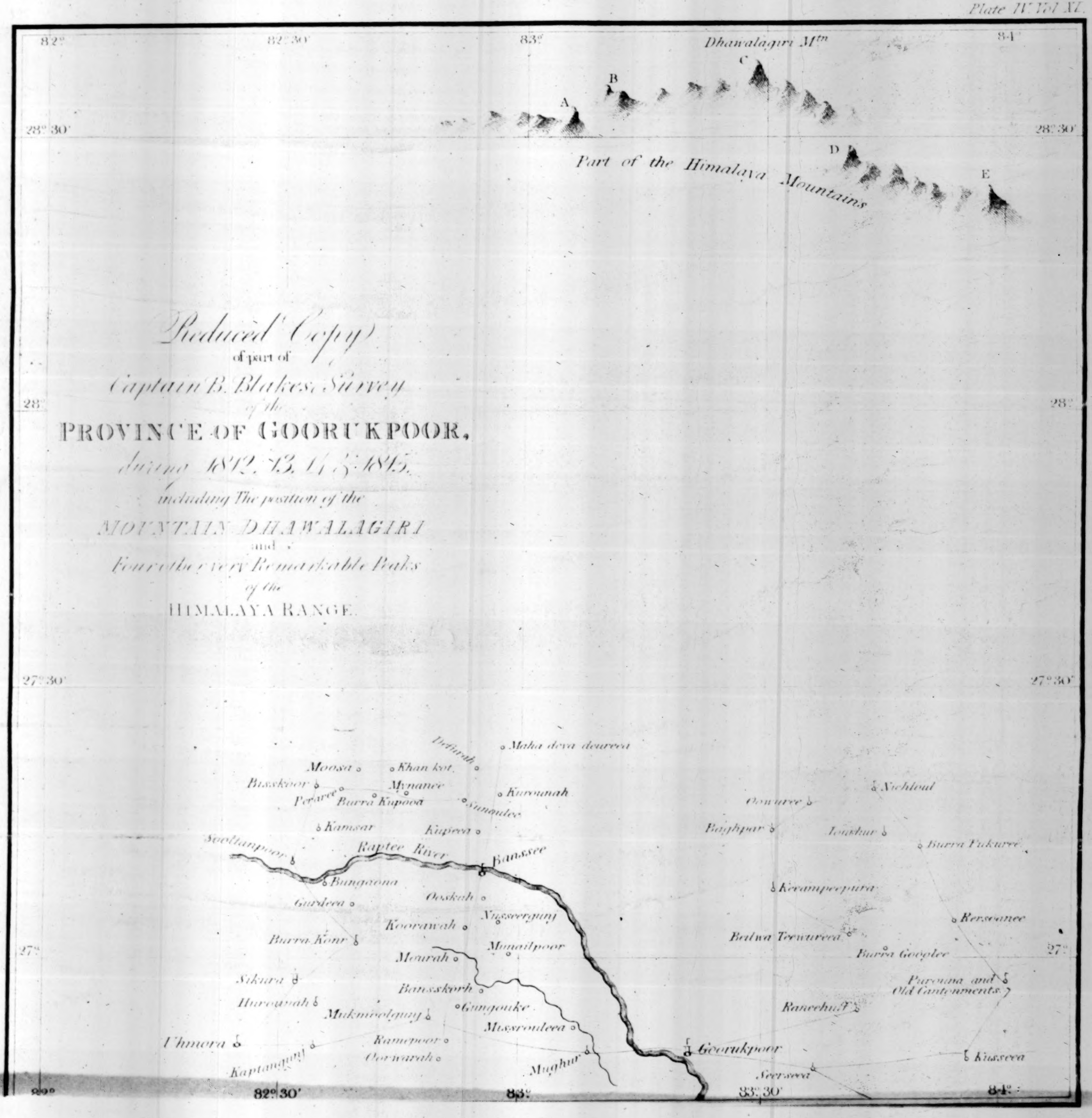
Plate IV. Vol XI.



Reduced Copy
of part of
Captain B. Blake's Survey
of the
PROVINCE OF GOORUKPOOR,
during 1812, 13, 14 & 1815.
including The position of the
MOUNTAIN DHAWALAGIRI
and
Four other very Remarkable Peaks
of the
HIMALAYA RANGE.

GLASGOW
PHILOSOPHICAL
SOCIETY.

Art. XVIII. METEOROLOGICAL DIARY for the Months of March, April, and May, 1821, kept at Earl Spencer's Seat at Althorp, in Northamptonshire. The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

For March, 1821.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Thursday - -	1	29	41	29,19	29,25	SE	EbS
Friday - - - -	2	31	48	29,59	29,61	SW	SE
Saturday - -	3	41	51	29,57	29,50	E	SW
Sunday - - -	4	42	54	29,46	29,46	WbS	WbS
Monday - - -	5	34	34,5	29,70	29,80	NE	ESE
Tuesday - -	6	30	37	29,70	29,40	SE	EbS
Wednesday -	7	33	52	29,38	29,37	W	SW
Thursday - -	8	40	54	29,10	29,03	W	W
Friday - - -	9	37	52	29,40	29,38	W	SW
Saturday - -	10	39	58	29,38	29,48	W	W
Sunday - - -	11	38	57	29,64	29,68	W	W
Monday - - -	12	34	55	29,74	29,74	W	W
Tuesday - - -	13	33	57,5	29,81	29,83	WbS	W
Wednesday -	14	37	52,5	29,90	30,02	WbN	NW
Thursday - -	15	26	54	30,19	30,19	NE	WbS
Friday - - - -	16	29	50	30,17	30,66	WbS	SSW
Saturday - -	17	25,5	54	29,92	29,69	WSW	WbS
Sunday - - -	18	37	50,5	29,33	29,23	SW	W
Monday - - -	19	33	44	29,09	29,08	WbN	WbN
Tuesday - - -	20	34	46	29,14	29,17	WbN	WbN
Wednesday -	21	33	45	29,12	29,27	W	NNW
Thursday - -	22	35	44	29,50	29,73	NW	NNE
Friday - - - -	23	27	49	29,90	29,88	NW	WbN
Saturday - -	24	34	45	29,65	29,41	SSW	SW
Sunday - - -	25	38	53	29,23	29,32	W	NW
Monday - - -	26	31,5	55	29,43	29,40	WSW	SbE
Tuesday - - -	27	39	43,5	29,00	29,14	SE	SW
Wednesday -	28	32	46	29,07	28,97	E	SE
Thursday - -	29	37	46,5	28,97	29,06	SE	WbN
Friday - - - -	30	33	56	29,46	29,50	WbN	SW
Saturday - -	31	38	54	29,27	29,27	SW	WbS

For April, 1821.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Sunday - - -	1	32	56	29,47	29,41	W	SW
Monday - - -	2	42	57	29,11	29,09	W	WbS
Tuesday - - -	3	37	54	29,09	29,08	SW	SW
Wednesday -	4	36	56	29,15	29,19	SW	WbN
Thursday - -	5	35	52	29,32	29,60	WbN	NW
Friday - - - -	6	30	49	29,84	29,88	W	W
Saturday - -	7	41,5	58	29,80	29,90	WbN	WbN
Sunday - - -	8	43	65,5	29,99	29,98	W	W
Monday - - -	9	41	68	29,90	29,78	WbS	W
Tuesday - - -	10	49	64	29,70	29,60	W	W
Wednesday -	11	44	60	29,51	29,36	W	SW
Thursday - -	12	39	54	29,22	29,11	SW	SW
Friday - - - -	13	38	53	29,35	29,35	SW	SW
Saturday - -	14	35	48	29,50	29,10	SW	SSW
Sunday - - -	15	33	53	29,35	29,35	SW	SW
Monday - - -	16	31	57,5	29,35	23,30	SW	WbS
Tuesday - - -	17	32	68	29,31	29,35	W	W
Wednesday -	18	38	63	29,48	29,58	W	W
Thursday - -	19	37	53,5	29,57	29,33	SbE	SbE
Friday - - - -	20	45	61	29,39	29,47	WbS	SE
Saturday - -	21	47	59	29,60	29,82	NNE	NE
Sunday - - -	22	42	53,5	29,90	29,74	ENE	E
Monday - - -	23	41	67	29,48	29,38	E	E
Tuesday - - -	24	52	67,5	29,20	29,39	SE	SW
Wednesday -	25	41	72,5	29,51	29,53	SE	SE
Thursday - -	26	43	74	29,64	29,58	SE	NE
Friday	27	51	66	29,56	29,60	NW	W
Saturday	28	41	68	29,68	29,70	W	NbE
Sunday - - -	29	45	64,5	29,73	29,78	NW	SW
Monday - - -	30	48	56	29,90	29,94	NE	NE

For May, 1821.

		Thermometer		Barometer		Wind	
		Low	High	Morn.	Eve.	Morn.	Eve.
Tuesday - - -	1	33	54,5	29,94	29,82	ENE	N
Wednesday -	2	39	66	29,78	29,70	WNW	ESE
Thursday - -	3	49	66,5	29,63	29,57	SW	S
Friday - - - -	4	46	71	29,57	29,57	ESE	SW
Saturday - -	5	45	66	29,53	29,39	SW	SW
Sunday - - -	6	42	63,5	29,32	29,40	WSW	SSW
Monday - - -	7	39	62,5	29,68	29,80	WbS	SSW
Tuesday - - -	8	51	70	29,80	29,88	WbS	W
Wednesday -	9	39,5	69	30,00	30,07	W	W
Thursday - -	10	42	62	30,11	30,08	W	W
Friday - - - -	11	48	65	29,90	29,85	W	W
Saturday - -	12	51	63	29,73	29,60	NW	W
Sunday - - -	13	39	63,5	29,28	29,23	W	W
Monday - - -	14	40	59	29,20	29,22	W	W
Tuesday - - -	15	38	62	29,08	29,12	SW	W
Wednesday -	16	39	64	29,52	29,67	WbS	WbS
Thursday - -	17	39	63	29,80	29,77	WbS	SE
Friday - - - -	13	44	64	29,77	29,90	NW	WbN
Saturday - -	19	36,5	67	30,10	30,09	WbN	WbN
Sunday - - -	20	41,5	57	30,09	30,09	NE	NE
Monday - - -	21	31	60	30,06	29,98	ENE	E
Tuesday - - -	22	34	55	29,94	29,87	ENE	ENE
Wednesday -	23	36	53	29,76	29,74	E	ENE
Thursday - -	24	34	57	29,83	29,87	NbW	NNW
Friday - - - -	25	35,5	66	29,84	29,60	WbS	WNW
Saturday - -	26	34,5	58	29,62	29,68	W	W
Sunday - - -	27	32	61,5	29,78	29,78	NW	WbN
Monday - - -	28	42	60,5	29,78	29,80	NW	NW
Tuesday - - -	29	36	65	29,94	30,00	WbS	NW
Wednesday -	30	37	64	30,09	30,08	ENE	ENE
Thursday - -	31	38	62	30,08	30,00	ESE	ESE

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